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VOLUME 58 MARCH 1950 NUMBER 2

TURBIDITY CURRENTS AS A CAUSE OF GRADED BEDDING
PH. H. KUENEN AND C. I. MIGLIORINI 91

THE CAROLINA "BAYS" . . . . . FRANK A. MELTON 128

SYNTHETIC SAPPHIRINE AND ITS STABILITY RELATIONS IN THE SYSTEM MgO-Al_2O_3-SiO_2 . . . . . WILFRID R. FOSTER 135

GEOLOGICAL NOTES

CURRENT-RIPPLE PROFILES AND THEIR DEVELOPMENT . . . . . HENRY W. MENARD 152

SOME UNUSUAL RIPPLE MARKS FROM THE TRIASSIC OF UTAH . . . . . WM. LEE STOKES 153

AN OBSERVATION BEARING ON THE PROBLEM OF EXFOLIATION . . ARTHUR DAVID HOWARD 155

ON THE BIAS OF GRAIN-SIZE MEASUREMENTS MADE IN THIN SECTION . . . . F. CHAYES 156

GRAIN-SIZE MEASUREMENTS MADE IN THIN SECTION: COMMENTS . . . . W. C. KRUMBEIN 160

DISCUSSION

SUBMARINE CANYONS AND THE ICE AGE: A DISCUSSION . . . . . O. D. VON ENGELN 161

L'OXYGÈNE EN LES CALCULS PÉTROGRAPHIQUES: UNE DISCUSSION . . . . . RENÉ PERRIN 163

TURBIDITY CURRENTS AND GRAYWACKES: A DISCUSSION . . . . . F. J. PETTIJOHN 169

REVIEWS 172

PUBLICATIONS RECEIVED 177

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TURBIDITY CURRENTS AS A CAUSE OF GRADED BEDDING[1]

PH. H. KUENEN[2] AND C. I. MIGLIORINI[3]

ABSTRACT

Some types of graded bedding, especially minor or isolated occurrences and varved "clays," can be readily accounted for by normal processes of sedimentation. Volcanic eruptions, dust storms, annual and longer climatic rhythms, rejuvenation of relief at the source or filling in of the sedimentary basin, churning up of sediment by storm waves, are among the more obvious potential causes of graded deposits.

But the majority of occurrences cannot be explained by any of these processes. The authors believe turbidity currents of high density may be invoked to have supplied the sediment and deposited it in graded beds.

The absence or insignificance of current bedding and ripple marking in graded deposits, the deposition of coarse material on the unconsolidated fine-grained top of the preceding bed, the enormous extent of each individual member without apparent change in thickness or character, and the frequent inclusion of angular fragments (even composed of clay) and of redeposited fossils are among the chief characteristics opposing transport and deposition by normal agents. All these and several other features, such as the often observed combination of grading with slumping, are explained by turbidity currents of high density. The graded graywacke series of some geosynclines, coarse graded conglomerates and breccias deposited in deep water, and some deep-sea sands can be accounted for in this manner. Experimental investigations on the nature of turbidity currents and the production of graded bedding are described, and the formation of certain graded rocks from the Apennines are treated in detail. Finally, an attempt is made to give a general elucidation of the formation of graded sediments by the action of turbidity currents of high density.

INTRODUCTION

GENERAL

The present authors arrived independently at the conclusion that the most important types of graded bedding appear to have been produced by the action of turbidity currents of high density on the sea floor. But, whereas one of us (Migliorini) was engaged in the investigation of graded rocks encountered in the field, the other (Kuenen) had studied artificial turbidity currents of high density in the laboratory. When they found how closely their conclusions tallied, although arrived at from opposite starting points, they decided to present their results jointly to a wider public. They have since discovered that Bramlette and Bradley (1940) had already indicated the same process as explanation of some graded beds encountered in recent deep-sea deposits.

A general description of graded bedding will first be given, followed by a review of possible processes leading to this type of deposition. Then experimental evidence on the properties of turbidity currents and their more direct bearing on the problem of graded deposits will be presented. Afterward, two different types of graded deposits from the Apennines will be described, and it will be shown how the observed puzzling features may

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be explained by turbidity currents of high density. Finally, a more general picture will be outlined of the hypothesis here proposed, showing its scope and limitations. This method of presentation renders a certain amount of repetition unavoidable. The writers hope, however, that it may have the advantage of bringing out more clearly not only how the two different lines of approach (through experimental data and through field evidence) lead to the same general result but also how the combination provides a more complete view than could be attained by following either of the two approaches separately.

GRADED BEDDING

Graded bedding has been observed and described many times in the past, although this term was not generally applied until 1930, when E. B. Bailey wrote his now classic article on sedimentation and tectonics. Bailey applied this term to beds which show a gradual transition in grain size from coarse at the bottom to fine at the top. He excluded the beds with current bedding in which the coarser sand grains or pebbles have rolled to the bottom of each new foreset lamina so as to produce an aggregate effect upon the constitution of a composite current bed as a whole. Since then, interest in graded bedding has greatly increased, partly because of its value in distinguishing top and bottom of nonfossiliferous beds in disturbed structures (especially pre-Cambrian) (Shrock, 1948, p. 78).

Grading is observed in a wide variety of deposits. It may be encountered in coarse conglomerates or breccias, in sands, silts, or clays, or in beds with particles ranging between the extremes in size. Graded sandstones are normally impure and muddy throughout. The primary clay matrix may equal or exceed the sand component (Woodland, 1938;[4] Pettijohn, 1948). Individual graded beds may range from a few millimeters to several meters in thickness. Nearly always the coarsest grade is found at the bottom of a graded bed, but exceptions occur. In some localities a single or several graded beds are intercalated between beds composed of similar or entirely different materials, but unsorted. In other cases graded beds occur in thick series to the exclusion of ungraded deposits. On the one hand, the beds may be rather similar in thickness and composition, as in varved series, although in these there is generally a gradual upward trend toward finer and thinner beds. On the other hand, the thickness and grain size may show important arbitrary variations, and in those cases the thicker beds are, in general, the coarser ones (Henderson, 1943). The graded sandstones of the pre-Cambrian are of the latter type. Usually the sandy portion of each bed is several times thicker than the argillite member. Pettijohn (1943, p. 954) reported that in the graded Archean graywackes intraformational shale- or slate-pebble conglomerates are commonly found at the base of the thicker graywacke beds.

A typical property of many graded series is the vast extent and constancy in thickness of each individual bed. In a large exposure no variation either in composition or in thickness need appear, even where the graded beds are no more than a few inches thick. These properties are by no means universal, however.

Another general characteristic is the absence of current bedding in graded series. Although there are many exceptions, the two features tend to be mutually exclusive. In fact, Bailey (1936) goes

[4] Woodland did not observe graded bedding in the Harlech Grit series, but Bailey (1930) says it is widespread.

so far as to say that graded bedding and cross-bedding are typical of two different environments of sedimentation. The former occurs in the deeper water of geosynclinal series and is commonly developed in graywackes (muddy sandstones). It is also commonly associated with slump structures. Cross-bedding, on the other hand, tends to occur in clean sandstones deposited in shallow marine waters or in rivers.

Some graded beds lie on a surface showing no signs of erosion, especially those beds that occur in a series of great thickness and vast extent. Others are deposited in scour channels or on a flat surface with obvious erosion features or ripple marks.

The particles of graded sandstones are generally rounded, those of graywackes are more angular. In coarser deposits the pebbles may be rounded, but in other cases they are subangular or even entirely unrounded. The latter is especially remarkable when the chunks consist of clay that was unconsolidated at the time of redeposition, proved by their being bent or warped. Some graded beds of an exceptional nature will be described in a later paragraph (a large number of references is given by Shrock, 1948).

CAUSES OF GRADING

Several causes have been suggested to account for graded bedding, all of which have probably been active in one case or another. The more obvious are as follows:

A. When sediment of a wide range of grain sizes is spasmodically introduced into a not too shallow body of water, it will tend each time to form a graded bed on the bottom. The larger particles sink more rapidly than the smaller and accumulate first. They are followed by continuously decreasing grade sizes until the smallest particles finally settle on top (Fossa-Mancini, 1922; Bailey, 1930, p. 88). Volcanic eruptions, dust storms, churning storm waves, and flood water spreading out in a thin layer over sea water (Bailey, 1930, p. 88) are among the agents that can supply the material in the manner required. Except for volcanic explosions, their action is limited to fine sand, silt, and lutite grades. In the case of churning storm waves, bottom erosion must precede the deposition, although transport by currents may have carried the material to a locality beyond the erosive action and redeposited it on an uneroded surface.

B. With a different category of causes, the grading is due to a gradual decrease in transporting power of the supplying agent. The variation may be rapid, for example when a river decreases in volume during the dry season or during the cold season in an arctic region. The variation is slow when (1) a rejuvenated river erodes and smooths down its bed, thus carrying to the coast a load continuously decreasing in grain size; (2) coastal erosion cuts deeper into the land mass, and supply of sediment at a point farther off shore shows a similar trend; (3) silting up of a basin or shallowing due to elevation lessens the declivity of the subaquatic slope and thus reduces the transporting power along the bottom; (4) subsidence of the sea floor results in weaker wave action and hence in the supply of smaller grains; and (5) a change in climate or physiographic developments either on land or in the sea adversely influences the transporting power of rivers or marine currents.

Whereas in the first category (A) grading must necessarily be from coarse to fine upward, it may be found in the second category (B) to go from coarse to fine in the opposite direction. However,

this is very seldom found to be the case. There are several reasons, but the main one is doubtless that increasing power of transport needed to cause inverted grading must generally result in scour and nondeposition.

The deposition of some graded deposits can be readily explained by one of the above causes. But in other cases difficulties are encountered that will be dealt with in detail later. Hence, other than normal processes have been invoked to render the deposition of graded bedding understandable.

Bailey (1930, p. 88) says: "Graded sandstones are products of settling through water under conditions which allow the larger particles to reach the bottom in advance of their smaller comrades," and (p. 89) "... I suggest that graded sandstone beds are often the records of sea-quakes. The coastal fringe of unconsolidated current-carried sand and mud would provide a source for the material." The exact nature of the process is not elaborated. But in a different connection, when dealing with the formation of submarine canyons (1938), he put forward the suggestion that submarine landslides caused by earthquakes would readily merge into submarine mud rivers. The combination of these two suggestions would lead to the same explanation of graded bedding as is advocated by the present authors. Stetson and Smith (1938) and Bell (1942) pointed to the importance of turbidity currents in transporting material, but they did not consider the nature of the deposit or the possibility of currents of high density.

The view that graded deposits may have been produced by turbidity currents was first definitely put forward in 1940 by Bramlette and Bradley in connection with certain beds encountered in recent deep-sea cores. They found that in one case a layer about 8 cm. thick contrasts markedly with the underlying homogeneous clayey sediment, which contains very few Foraminifera, in that it consists of a mixture of sand and Foraminifera with clay pellets, but otherwise very little fine-grained material, and is decidedly friable. It is graded from coarsest at the base (average diameter of clastic grains 0.45 mm.) to fine at the top (0.06 mm.). The Foraminifera belong to a warmer period than those in the adjoining clay and are evidently *remaniés*. These features (p. 16) "suggest that the sample consists of material thrown into suspension by a submarine slump, carried beyond the slide itself, and deposited rapidly." They point out that A. Heim showed how clouds of suspended sediment traveled far beyond the outermost limit of certain subaqueous slides in lakes. Bramlette and Bradley did not state how great they suppose the density of the flow to have been, but they appear to have had in mind rather dilute suspensions because they refer to "clouds of suspended sediment." Neither is the suggestion applied to graded bedding in general.

EXPERIMENTAL INVESTIGATION

NATURE OF TURBIDITY CURRENTS

Turbidity currents ("suspension currents" is synonymous) are currents with suspended sediment flowing along the floor of a standing body of water owing to their higher density. The term "density currents" has been used (also by Kuenen) but is less appropriate because greater density may be due to lower temperature or higher salinity; in fact, most currents in the oceans are "density currents," but these are not what geological authors using this term mean.

Turbidity currents may leave the bottom and spread out horizontally when, on their downward path, they encounter water of a density greater than their own. They may also flow with an upward component, owing to momentum upon reaching a counterslope. In reservoirs and lakes the turbid water may come to rest in the deeper parts and assume a level surface. It is doubtful whether this ponding is of importance in marine waters because the flocculated lutite particles will settle out as the water loses its turbulence.

Turbidity currents have been observed in reservoirs, and, in 1936, Daly (see 1942) invoked their action to account for the development of submarine canyons. Kuenen (1937, 1938) experimented with such currents, and later Bell (1942) also studied their flow in the laboratory in connection with the occurrence of turbidity currents in reservoirs.

Afterward, Kuenen (1947) suggested the possibility that currents of much higher density, carrying in suspension not only clay and silt but also coarser grades, might likewise occur in nature. He subsequently made an experimental study of such high-density currents to ascertain their transporting competency and general properties of flow. The results were reported at the International Geological Congress in 1948, and the opinion was offered that, besides being active in the formation of submarine canyons, they may have produced graded bedding. The interest shown by several colleagues in this latter suggestion induced him also to study the deposits made by experimental flows (preliminary report in Dutch, Kuenen, 1948).

SMALL-SCALE EXPERIMENTS

The artificial turbidity currents were first produced on a small scale in the following manner: Various mixtures of fine gravel, sand, and clay were churned up with water in a bucket of 20 liters volume, the bottom of which could be slid away sideways. This suspension was released at the higher end of an aquarium 2 meters long and slightly tilted lengthwise. On the floor was a layer of gypsum giving a slope toward one of the long sides, so that the current traveled along a channel (fig. 1, pl. 1, *A*). By varying the slope, volume, density, and viscosity (more or less clay), a wide range of conditions was examined. The velocity was measured by stop watch over 0.6 and 1.2 meters of flow. Values of 80–90 cm. per second were attained when density was 2, slope 17°, and volume 18 liters. Owing to various complicating factors, theoretical treatment of the velocity of flow is difficult and will not be attempted here.

It was found that a silty clay suspension with a density of 1.54 (one part water against 1½ parts clay by weight[5]) will slide down the channel, when it slopes at 5°, with a movement resembling that of a glacier, although much swifter. At a certain point it stops abruptly on the slope. Somewhat more mobile suspensions will accumulate as a sludge at the bottom of the declivity. If the density is about 1.45 or less, and hence the mobility greater, the mass will flow along the channel in a turbulent current and will pass around a sharp corner or mount over an obstacle.

In the suspension, part of the clay can be replaced by a much larger amount of sand without altering the viscosity. The density is thereby increased. The maximum density thus attainable in a suspen-

[5] In the following portions of the paper, when amounts of clay and fine and coarse sand are given, this refers to the weights of the various types of material used. In table 1 (p. 103) the compositions are given, and it should be noted that the "clay" is material of which only 62 per cent is smaller than 4 μ.

sion that will flow turbulently is about 2, the suspension consisting of water, clay, sand, and fine gravel in the proportion, for example, of 5:3:8:12 or 5:5.5:2.5:14.

Although there exists a gradual transition from a thick paste with a density of, say, 2.2 to a mobile watery suspension, the changeover from a sliding movement to turbulent flow occurs at a fairly sharp

A remarkable property of high-density turbidity currents is their astounding transporting competency. It was shown on theoretical grounds and experimentally that a current of density 2 can roll along the bottom fragments several thousand times (± 10,000) the weight of the maximum grains shifted by a current of clear water with the same velocity.

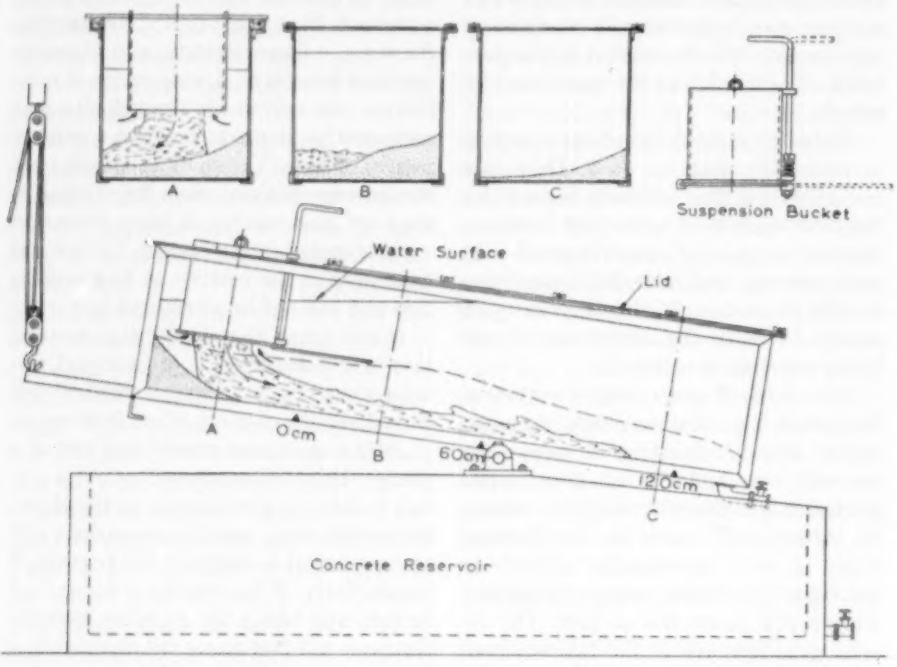

FIG. 1.—Tank with suspension bucket as used for studying small-scale turbidity currents of high density

critical point in this series. The exact density at this point depends on the relative amount and nature of the clay and coarser fractions used. The volume of the flow and the bottom slope probably play a part also. The greater the amount and the steeper the declivity, the higher the velocity will be, and, presumably, the more readily turbulence will develop. No appreciable difference was noticed when the suspension was made with water containing 4 per cent sodium chloride.

This capacity falls off rapidly with decreasing density. For a density of 1.5, the factor has already dropped to a few hundred.

The maximum size of grains retained in suspension was not studied, but the fact that a current of 50 cm. per second and density 1.9 carries small pebbles in suspension is sufficient evidence that in this mode of transport the competency is also great. Naturally, the load is likewise exceptionally high.

Saltation, on the other hand, is reduced to the minimum. The internal friction of the suspension is so great that, even with high velocities, colliding grains cannot rebound. In fact, they do not even sink in a stationary suspension of high viscosity.

GRADED DEPOSITS

A slight alteration in the setup of the experiments was made for studying the deposition. In order to reduce the distance of flow, the aquarium was not tilted, and the current depended merely on the initial momentum acquired by the suspension dropping from the bucket to the floor of the tank. Only small volumes of suspension, 3 or 4 liters, were used. In addition, a partition was placed in the aquarium, running to within a short distance of the far end (fig. 2). The flow, passing around it, ran back along the other side. As soon as it returned, the opening in the partition was closed by a sash door. By this means the originally deposited bed in front of the partition was covered only to a small extent by mud deposited from the return flow.

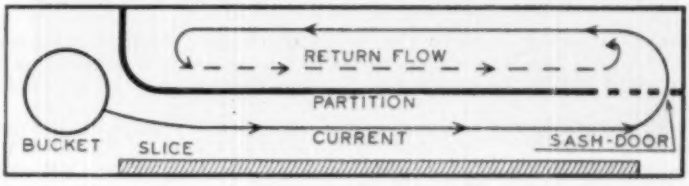

FIG. 2.—Tank viewed from above, showing partition and sash door, which is closed when the flow returns. The position of the slice of sediment from which a nitrocellulose film was made is shown.

Several flows were released in succession to build up a series of beds. It was found that an interval of a couple of days was required to allow sufficient settling and compaction of the upper, watery part of the deposit. Otherwise, the following current tended to wash away this fine material.

Owing to the interstitial mud, the sand grains can hardly be distinguished through the glass of the aquarium, and grading is not obvious. Therefore, the water was next drawn off and the deposited series allowed to dry slowly. When sufficiently firm, the sedimentary layer was removed except for a slice a few centimeters thick along the side. From this slice a nitrocellulose film was made to obtain a vertical lengthwise section. The mass is first impregnated with dilute nitrocellulose and then painted over with a few layers of flexible cellulose varnish. The film is then pulled off and cleaned on the sediment side with a brush or under a running tap. The interstitial mud is sufficiently removed by this means to allow the larger grains to protrude.

The film thus produced, although obscuring the muddy character of the sandy parts, gives an excellent picture of the distribution of grain size of the sand fractions. In each stratum the coarse material was found to be markedly graded, both horizontally and vertically (pl. 1, *B*).

There is no indication of ripple marking, not even when the finer upper part had been eroded by the next flow. It is believed that the mechanism producing ripple marks cannot readily occur in the viscous suspensions. In clear water a thin film of grains rolls along the bottom and builds up minute foreset laminae on the developing ripples. In the turbidity currents the grains very nearly float in

suspension. They do not roll over the edge of irregularities but are wafted some distance beyond. Hence depressions are not filled progressively from the upstream end, as they are in ripple marking, but over the entire length. In this manner a smooth bottom develops during aggradation. Possibly ripple marks may form from turbidity currents of intermediate density in which the grains are not so well supported.

The grading is caused by a different mechanism than operates when sediment is allowed to settle through a column of stagnant water, in which case there is not necessarily any horizontal grading. Only

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PLATE 1

A, Turbidity current in tilted aquarium.

B, Five graded beds produced in small-scale experiments at 60 cm. from starting point (slightly enlarged). Note coarse grains and absence of grading in light bed near top, for reasons explained in text.

C, Ditch used in large-scale experiments with drying deposit. Over-all length 30 meters. Suspension box with sash door at near end. Glass tubes are fixed to the brackets sticking out from the bank when the velocity is to be measured. A sample has been taken just beyond the first measuring point.

PLATE 2

A, Between thin finer beds (the upper one with a few angular chunks of plastic white clay) lies a thick deposit formed by a mudflow of sufficient mobility to have been on the verge of turbulent flow (6 liters water, 9 kg. clay, 8 kg. gravel, slope 8°). Upper section 50 cm., lower section 175 cm. from origin. No horizontal or vertical grading.

B, Three vertical lengthwise sections of deposits formed in large-scale experiments. *Top:* three fine silt-clay beds resembling varved clays, lying on thicker graded bed of fine sand to clay, experiment A. Upper section 17 meters from origin; middle section 20 meters from origin (note slump structure in two middle beds); lower section 23 meters from origin. All three slightly enlarged.

PLATE 3

A, Two sections of deposits, experiment A. Upper 7.5, lower 8 meters, from origin. Note absence of current bedding, ripple marking, or horizontal grading. The lower section shows slump structures. The photographs show parts of the lower section of pl. 4, *B*.

B, Detail of beds in top section of *A*, showing excellent vertical grading (centimeter scale on left).

PLATE 4

A, Two sections of deposits, experiment A, 2 meters from origin. Arrows show direction of flow. Note slump structures.

B, Two sections of deposits, experiment A, both $7\frac{1}{2}$–$8\frac{1}{2}$ meters from origin. Upper two fit together at letter *A*, lower at letter *B*. Apart from slump structures and slight warping due to drying, the beds are very regular and well graded.

C. Brecciola beds with mudstone intercalations, left bank of the Torrente Corsonna by the Molino Gasperetti, near Barga, Province of Lucca, Italy.

PLATE 5

A, Undersurface of a macigno sandstone bed with counterimpressions of wormlike tracks. Quarry at Maiano, about 3 km. northeast of Florence, Italy. The ruler is 20 cm. long.

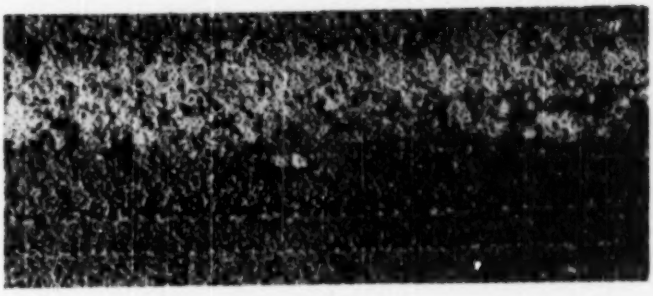
B, Fine gravel from a macigno sandstone bed near Lucolena, Province of Florence, Italy (×2).

C, Fairly coarse brecciola cut parallel to the bedding. Bed of the Torrente Loppora, just under the track from Barga to Tiglio, Province of Lucca, Italy (×5).

D, Mudstone flakes in a macigno sandstone bed. The dashed line at the top right-hand corner marks the top of the bed. Quarry at Monte Ceceri, about 4 km. north of Florence, Italy. The match box measures 5 × 3.5 cm.

E, Pebbles from a macigno sandstone bed near Castiglioni, Valdarno Superiore, Province of Florence, Italy (× 0.5).

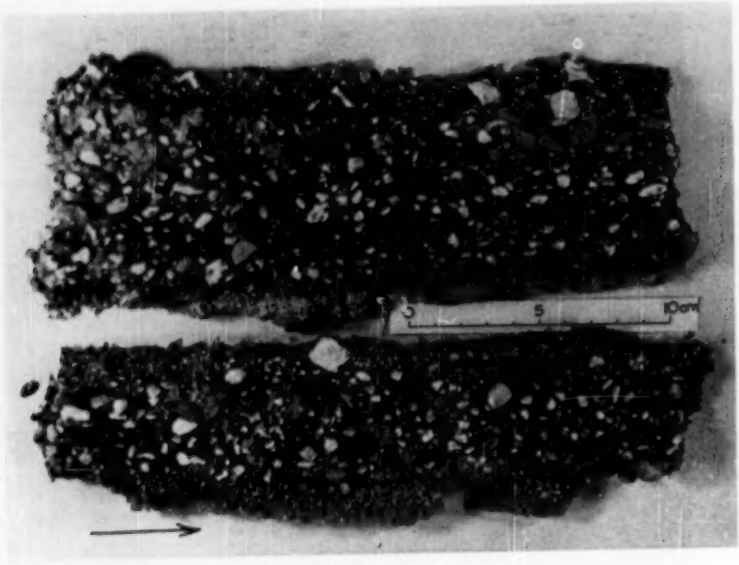
A



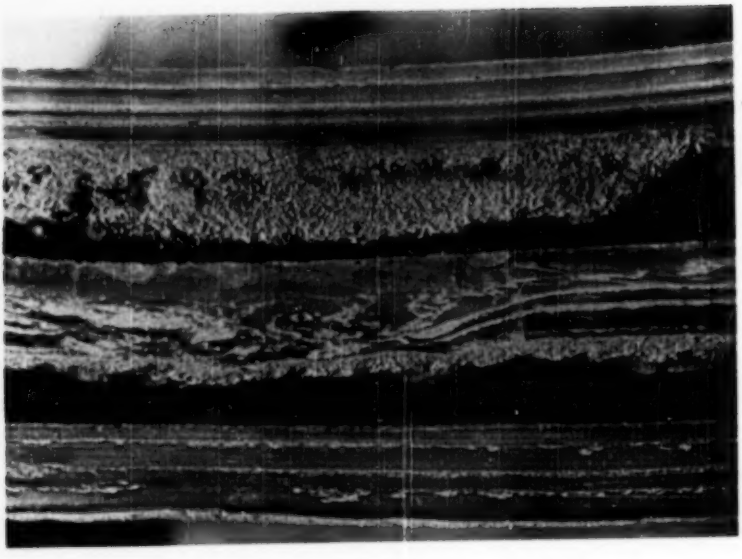
B

C

Experimental equipment; artificial graded beds

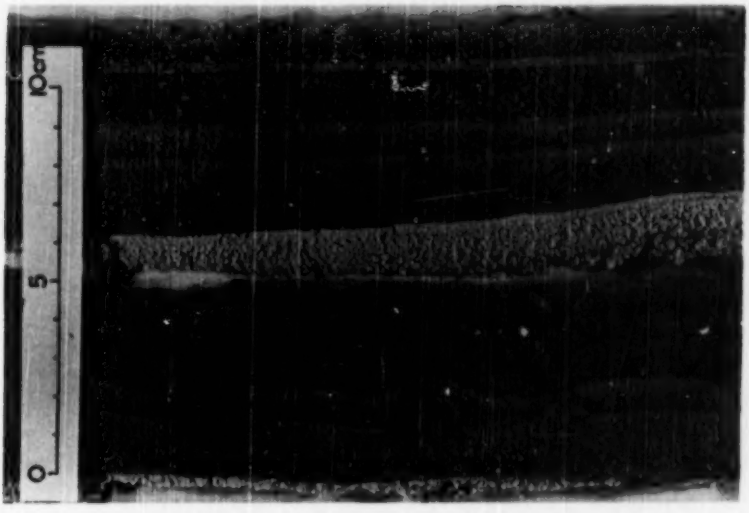


A

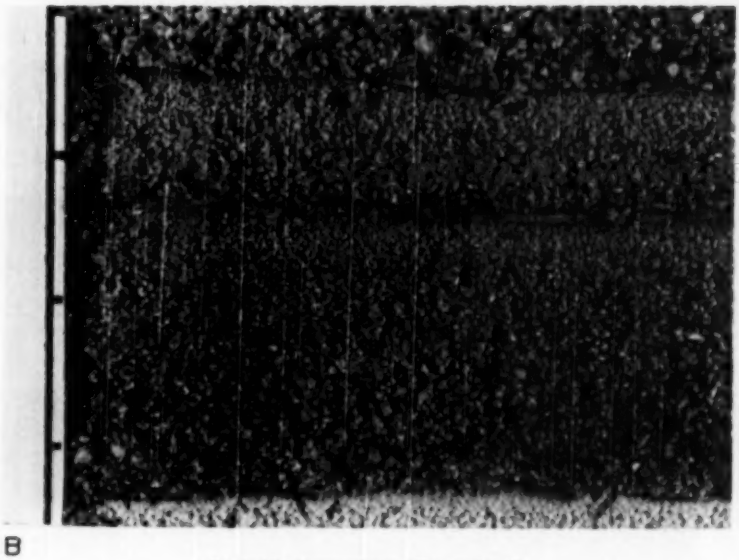


B

Mud flows and graded beds experimentally produced

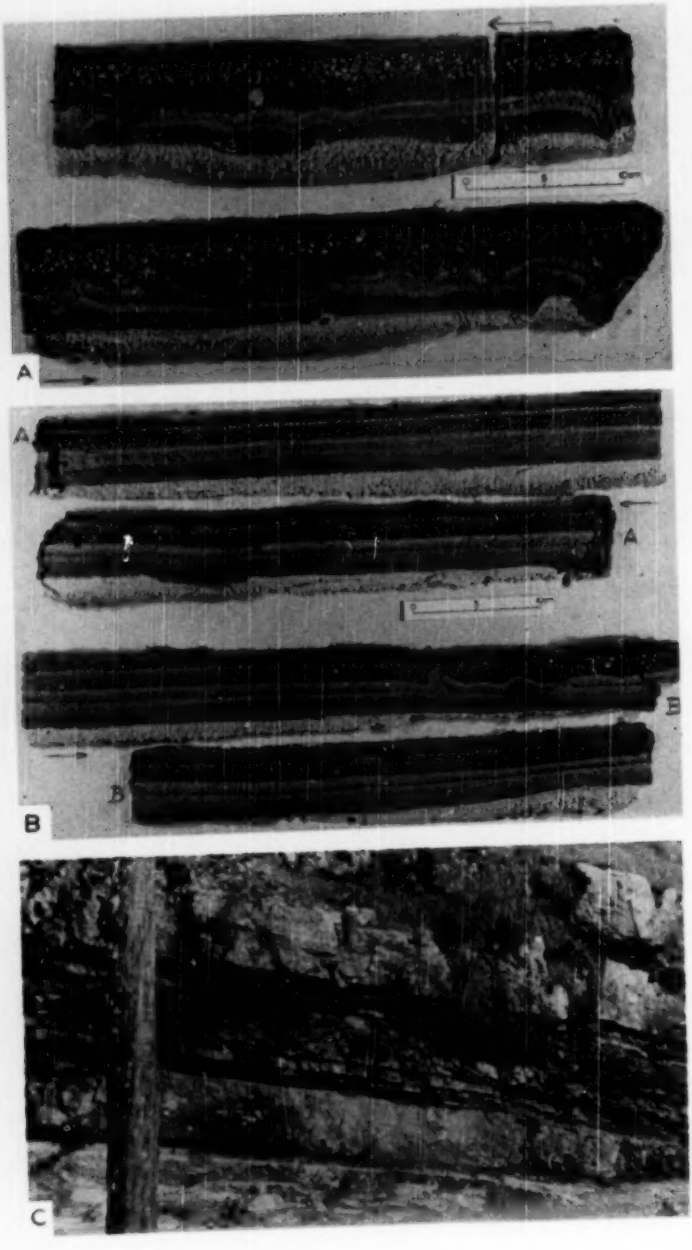


A



B

Graded beds experimentally produced



Artificial graded bedding; "brecciola" outcrop

A

B

C

D

E

Features of "macigno" sandstone and section of "brecciola"

if the mechanism by which the sediment is spread out over the surface of the basin has produced sorting, will horizontal grading occur in the deposit. In the case of turbidity currents the current is loaded to capacity or even overloaded. It starts to deposit the larger grains and, as it continues along the bottom, allows grades of decreasing size to settle farther and farther along. This decrease is partly due to loss of larger grains, leaving only smaller ones to be dropped, and partly to decreasing velocity and increasing dilution (loss of sediment and turbulent mixing with clear water from the tank) which allow fractions to settle which before could still be transported. Thus the horizontal grading is produced.

The vertical grading is due to a different phenomenon. Each part of the suspension that has been diluted, either by loss of sediment or by mixing with clear water, must tend to lag behind. Hence, after the main part of the current with maximum density has swept past, flow continues for a considerable time at the same spot but shows gradually more dilute suspension and flows at a continuously decreasing velocity. Consequently, at each point on the bottom, deposition takes place from a flow which is decreasing in competency from an initial maximum. Finally, a semitransparent cloud gradually clears away after depositing a small amount of lutite.

In one experimental flow a different kind of light-colored clay was used which formed a suspension showing a slightly higher internal friction. The heaviest part of the mass slid along the bottom in a thin layer and came to form a thin wedge, while the remainder produced a normal turbidity current. In this wedge the coarsest grains of sand had been carried farther away from the source than in the pure turbidity experiments, but the stratum was not graded.

LARGE-SCALE EXPERIMENTS

It soon became obvious that the tank was too small to release flows of greater volume than a few liters if the deposition and momentum were to be studied. Experiments on a much larger scale were therefore undertaken in a ditch dug in the garden of the Institute. The shape and dimensions are shown in figure 3 and plate 1, *C*. At one end a wooden box with a sash door was built in, to receive the suspension. The floor of the ditch sloped down 20 cm. over a distance of 2 meters and thence ran horizontally for a distance of 18 meters. A bend of 90° with a radius of 2 meters (length 3 meters) followed, and, after an additional horizontal stretch of 4 meters, the floor sloped upward to the surface over a distance of 4 meters. Thus the total length of the horizontal part of the floor was 25 meters. The depth of water in the ditch was 60 cm. It was coated with asphalt paper to render it watertight, a relatively cheap but not very satisfactory method. The center of gravity of the suspension in the box at the moment that the flow was released was 40–45 cm. above the horizontal part of the floor in the ditch; but, as the flow also showed a depth of from 10 to 20 cm., the average vertical drop producing the momentum may be put at 35 cm. over a distance of 225 cm. The volume of suspension used was generally 100–120 liters, the maximum 160 liters.

The velocity was measured over eight consecutive stretches of 3 meters. As the water was not sufficiently clear to see the flow from above, glass tubes were placed with their closed ends just above the bottom and the open ends well free of the water. By looking down the tube, one could accurately note the moment that the lower end became engulfed in the turbid current. An ordinary telegraph recorder was wound up and put in action

indoors, with a small synchrone motor arranged so as to give small dots on the recording strip every 1.6 seconds. The observer stood looking down the first tube at the lower end of the slope with a Morse key, connected with the telegraph, in his hand. At the moment the current was seen to pass, he made a short line on the recording strip by making the contact. He then ran to the next tube to note the moment of passing, and so on to the last tube, mounted 24 meters from the spade. The sand then added, and after a few minutes of churning the mass was ready for the experiment. At a signal the sash door was raised suddenly and the suspension released.

As long as the water level on both sides of the door was about the same, little commotion was produced, but if the level in the box was 5 or 10 cm. higher, a powerful translation wave ran along the ditch. As a result of this "tsunamic" wave, or at least together with it, a

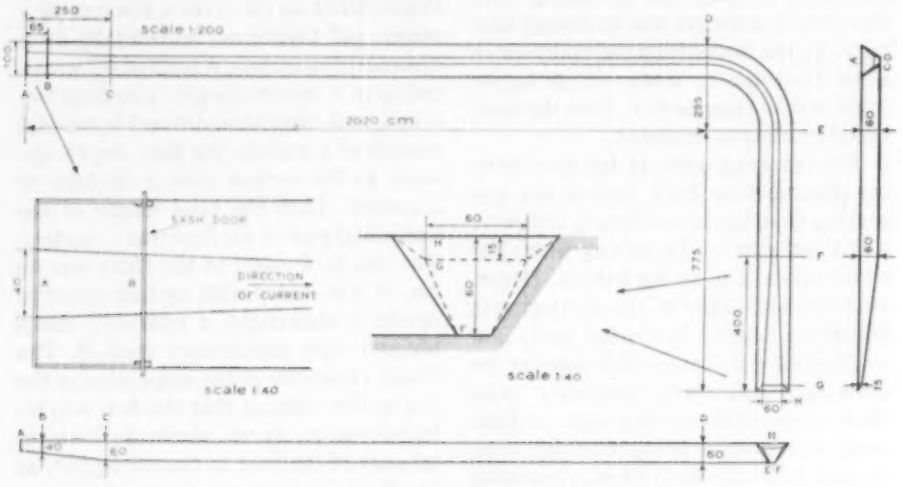


FIG. 3.—Ditch used for the larger-scale experiments

first. There the upward slope of the bottom was reached, and the current gradually came to rest while rising to within a few centimeters of the surface.

The suspensions were made by adding finely ground, low-grade pottery clay to water kept in motion by a simple electric churning device. After 1 or 2 hours of stirring, a perfect watery paste containing 35 kg. of clay in 32 liters of water was obtained. The required amount of this paste was poured into a measured amount of water in the suspension box at the head of the ditch and mixed with a seiche-like swinging of the water was noted, which rendered the velocity of the flow irregular. It was therefore avoided as far as possible after one or two experiments. However, the irregularities thus introduced rendered more accurate timing by film and chronometer superfluous. The measurement of velocities in the first series of five flows was carried out without time dots on the recording strip. It was found that the recorder ran at varying speed, hence the measurements were not trustworthy and have been discarded.

There is a large number of factors influencing the velocity of the flow and the distance that sand is transported. The volume, the specific gravity, the viscosity, the slope, the condition of the bottom (clean or with sand or clay in various degrees of compaction) are the principal variables. It soon appeared that slight variations in at least some of these factors have a profound influence. Moreover, the clay used was not sufficiently constant in composition to warrant equal viscosity when equal amounts were used. Hence the results of the first two series of five flows each, although instructive, were not sufficient to elucidate the influence of all factors, let alone to evolve a formula for calculating the velocity of a flow from its composition. Obviously, many series of experiments are required, in each of which all factors except one are kept constant, if one is to ascertain the influence of each variable on the nature of the flow. It will also be necessary to measure the viscosity of each suspension and the thickness of the flows and to analyze the deposit left behind along the floor of the ditch before anything like quantitative data can be obtained. These are required if an attempt is to be made to extrapolate with any degree of accuracy from the experiments to nature.

The experimental investigation will be carried on later, but in the meantime the results already obtained are well worth recording, especially as interest in these matters is growing. Others wishing to plan a program of research may profit by the experience gained at Groningen.

EXPERIMENTAL RESULTS

The points of most general interest to the problem of graded bedding are the high velocities, the degree of dilution, the great distance that sand is transported, the excellent grading of the beds deposited at both high and low velocities, the absence of ripple marks and cross-bedding, and the occurrence in some cases of slump structures (slip bedding) in the faintly sloping deposits.

Velocities.—As already mentioned, the average vertical drop causing the flow was only 35 cm. over a distance of 225 cm. (slope 9°). The veolcity over the first 3 meters was 74–83 cm. per second and showed remarkably small variations within the range of compositions used. In most cases the second 3 meters were covered at nearly equal velocity, 67–74 cm. per second. The velocity over the next 6 or 9 meters dropped swiftly to about 20 cm. per second, and, from that point on, the rate of flow decreased only slightly.

One cannot be sure that in these experiments the flows had obtained their maximum velocity for the slope used. But, judging from the small-scale experiments, one can feel fairly sure that they had. It should further be borne in mind that in our velocity measurements we are dealing with the displacement of a front and that, in the body of the flow following, a slightly higher velocity exists. If the supply of suspension were to be kept up for some time, it might be inferred that the velocity would increase slightly. In figure 4, the results of the second series of five flows is shown graphically.

The granular composition of the materials used is shown in table 1, and the compositions of the suspensions used in table 2.

It should be noted that, in consequence of deposition which was greatest at the near end, the initial slope was gradually reduced by about 10 cm., while at the far end only 1–2 cm. had been built up. The influence, however,

seems to have been slight, except perhaps in the case of experiment A, V.

Another point worth noting is that the flows passed around the bend without appreciable alteration in velocity or deposition. But it could be seen that they mounted some distance against the outer wall, so that at the end of the bend the surface of the flow sloped some 30°–45° toward the inner wall.

To one of the suspensions (A, IV) 4 per cent NaCl was added to see whether any marked differences are to be expected in turbidity currents in sea water. The influence appears to have been negligible. The turbulence is probably so great that flocculated clay is carried as easily as finely divided particles.

The flows are very much diluted during their passage along the ditch. Very roughly, an original volume of 160 liters increased to ten times as much before the flow came to rest. But, as the sand had been deposited, the clay suspension had become even more dilute. On the other hand, most of this addition went into the formation of a very thin suspension, so that the main current probably arrived at the far end with a clay content only several times as small per unit of volume as the original constitution. On a continuous slope the increasing volume would help to maintain the rate of flow. This would retard deposition but increase turbulence and mixing. Hence it cannot be said offhand what might hap-

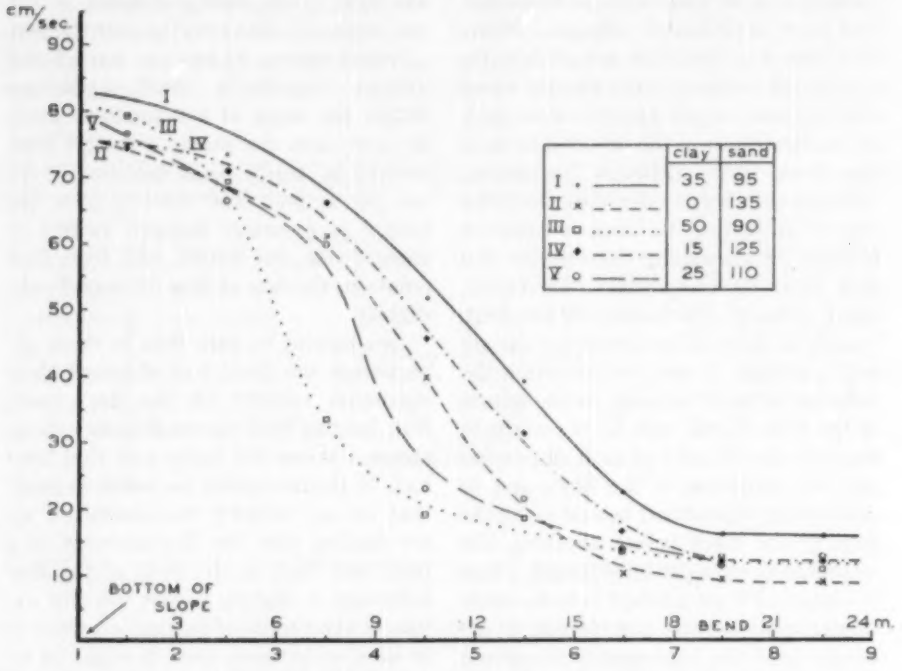


FIG. 4.—Measured velocities of experiment B, all with volume of about 112 liters and density 1.71–1.75. The points for timing, 1–9, with the distance from bottom of slope, are shown along the base. The position of the 90° bend in the ditch is shown.

pen in the way of dilution on a larger scale and on a continuous slope. Obviously, this aspect also requires further quantitative study. It is of great importance because the flow is changing in properties all the time, and these very properties determine the velocity of the current.

As the bottom slope in nature may be equal in many cases to the slope in the experiments from the sash door to the first measuring tube, whereas the thickness of the flow may be many times as large (estimated at 10–20 cm. in the experiments), one may expect considerably higher velocities to occur. The order of magnitude should be 2 meters per second for a thickness of 1 meter and 4 meters per second for a thickness of 4–5 meters.

Distance of flow and transport.—The experimental flows retained a velocity of more than 15 cm. per second even at a distance of 20 meters from the bottom of the slope. In nature, where vastly greater volumes must be operative, the flows should continue, after reaching a horizontal floor or a slight counterslope, for distances of quite a different order of magnitude. Distances of travel amounting to some kilometers or even many kilometers do not appear to be excluded.

Table 3 shows the thickness of the beds deposited along the ditch. Plates 2, 3, and 4, *A*, *B*, show sections made from the deposits of experiment A at various places. Hardly any sediment remained in the box.

Whereas all fine gravel was deposited within the first few meters, coarse sand was generally carried some 7–8 meters and in one case as far as 18 meters, and a thin layer of fine sand (100–150μ) was found in all deposits 20 meters away. The experiments show that some medium sand is transported as long as the velocity remains above 30–40 cm. per second, and some fine sand if velocities are above 15–20 cm. per second.

At first sight, these values of velocities and competency appear to compare unfavorably with the transporting competency of clear water in rivers (Hjulström, 1939; Nevin, 1946). In the latter case, however, we are dealing with the bottom load moved by rolling and saltation. The movement is slow as compared to that of the water, and the volume of the load is a minute fraction of the volume of the water required to move it. For transportation in suspension, much higher velocities are needed. In the experiments, on the other hand, we are dealing almost exclusively with transportation in suspension. It is effected in a few seconds by a small volume. The flow lasts so short a time that the transport by rolling can only be over a very small distance and may be neglected.

TABLE 1

GRAIN-SIZE ANALYSES OF MATERIALS USED

Size (μ)	Clay (Per Cent)	Size (μ)	Fine Sand (Per Cent)	Coarse Sand (Per Cent)
<2	54	<50	3	0.1
2–4	8	50–75	14	0.1
4–8	10	75–105	20	0.5
8–16	9	105–150	36	1.5
16–35	9	150–210	19	2
35–75	2	210–300	4	7
75–150	6	300–420	2	11
150–300	1	420–600	0.5	27
		600–850	0.5	34
		850–1190		15
		1190–1700		2

The artificial turbidity currents can give us some idea of what to expect in nature where the larger dimensions should maintain higher velocities. The conclusion is warranted that sand can be carried for great distances. In the artificial beds, no appreciable reduction in thickness of the beds can be detected

TABLE 2

Composition of Suspensions

	Water (Liters)	Clay (kg)	Fine Sand (kg)	Coarse Sand (kg)	Fine Gravel (kg)	Volume (Liters)	Density
Experiment A:							
I	60	.....	130	60	.....	135	1.84
II	90	32	75	75	.....	160	1.70
III	60	15	45	25	.....	93	1.56
IV	65*	35	11	58	.....	105	1.61
V	60	28	15	50	30	110	1.66
Experiment B:							
I	62	35	70	25	.....	112	1.71
II	60	.....	110	25	.....	112	1.74
III	60	50	70	20	.....	114	1.75
IV	60	15	100	25	.....	112	1.74
V	60	25	85	25	.....	113	1.73

* 4 per cent NaCl added.

TABLE 3

Thickness (in Millimeters) of Beds Formed in Experiments A (Dry) and B (Moist) at Distances from Lower End of Slope (in Meters)

Experiment A	Meters								
	0	3	6	9	12	15	18	21	24
V	25	20	11	8	1	2	3	2	2
IV	15	15	14	5	2	2	3	2	2
III	8	10	9	3	3	1	2	2	0.5
II	15	20	18	16	12	8	7	3	2
I*	23	24	18	19	11	0	.....	.....	.....
Sum	86	89	70	51	20	13	15	9	6.5

Experiment B	Meters								
	0	0.5	3	6	9	12	15	16.5	19.5
V	15	14	20	16.5	16	5	2	2.5	1.5
IV	27	23	14	16	14	10	5.5	2.5	2
III	36	28	30	16	3	2	2	3	1.5
II	17	20	19	17.5	17	9	2	1	0.5
I*	11	12	17.5	11	16	12	8	2.5	2.5
Sum	106	97	100.5	77	66	38	19.5	11.5	8

* Parts of both layers I were lost in taking samples; hence figures show minimum values.

over 1, and in cases even several, meters. Surely, one may assume that in a geosyncline, the cross-sectional dimensions of which are at least a thousand times the lengthwise dimensions of the experiments, individual beds could be laid down that were many kilometers wide and of apparently constant thickness even in large exposures.

Only tentative conclusions can be offered on the relations between the composition of the suspension and the distance of transport. Contrary to expectation, suspensions of pure sand (A, I, and B, II) carried their loads a long distance, almost as far as the optimum obtained with additional clay and farther than some clay-rich suspensions. The reason appears to be that the low viscosity enables swift flow, and, at the same time, turbulent friction with the stagnant water may have been smaller. On the other hand, the sand gives out more suddenly, once the stratum has thinned to a few millimeters (A, I). Adding clay to the suspension first causes increase of velocity and distance of transport to a maximum at about the composition of A, II, and B, I. Higher clay content causes a sudden retardation of the flow a few meters along the level floor and concomitant loss of nearly all the load. The viscosity influences the rate of flow adversely. The sand, however, is all carried in suspension, and a nongraded deposit of muddy sand is laid down (B, III). But it should be emphasized that these flows are traveling almost entirely by their momentum. If the bottom slope continued, it may be assumed that more viscous suspensions would retain a high velocity. Thus in the small-scale experiments, suspensions containing 2 parts water to $2\frac{3}{4}$ parts clay (= 1.7 times that in B, III) were seen to flow turbulently at 60 cm. per second on a slope of $8\frac{1}{2}°$. They would carry their loads much farther because the sand cannot sink. The success of the pure sand suspensions in these experiments is believed to be misleading. Even on a slope, the sand should settle quickly and the distance of transport fall far below that in a muddy flow.

For reasons that are not fully understood, the rate of flow shown by B, V, was so low as to approach that of the very clay-rich suspension in B, III. We will return to this matter when describing the deposits.

In the experiments the flows were hemmed in between steep walls, and in this respect they duplicate conditions in a submarine canyon, although they lack the continuous outward slope. On the slopes of a geosyncline, the suspensions could spread laterally and thus become thinner. On the other hand, the current might be set up on a proportionally much broader front, and this would tend to reduce the relative amount of spreading.

Grading.—The photographs here reproduced (pl. 3) may serve to demonstrate how excellent the grading can be. The very coarse material of experiment A, V, is not graded in a vertical sense, probably because it was dumped from an overloaded current too swiftly to be sorted. The same unsorted character is shown by coarse conglomerates in some California Tertiary basins. Whether this shows that these were carried by an overloaded turbidity current or by a mudflow is hard to say. Experimental mudflows under water in the aquarium of the small-scale experiments were found to leave entirely unsorted deposits (pl. 2, *A*). This was true even of suspensions containing medium-fine gravel and with a viscosity so low that they were on the verge of turbulent flow. As noted above,

even a turbulent flow (B, III) can deposit an unsorted stratum, provided that the viscosity is high.

One graded bed (A, II) was scraped off in six successive layers at two points, 1 meter apart (2.5 and 3.5 meters from the bottom of the slope). In figure 5, *1* and *2*, the grain-size analyzes are shown together with that of the suspension which formed the deposit. In *3* the analyses of experiment A, I, taken at the bottom of the slope, are shown. The lower 3 mm. are lacking. The thickness of each sample is given in millimeters measured from the base of the layer.

The excellence of the grading is remarkable, although actually the grading is more perfect than is apparent even from the graph. The layers successively scraped off were thicker than the grains, so that each analyzed sample consisted of a coarser lower part and a finer upper part, mixed together. Moreover, it was not possible to scrape exactly parallel to the lower surface of the layer because drying had slightly warped it.

The following points are worth noting. The percentage of fine material is relatively small in the coarse lower samples, but still we are dealing with "muddy sandstones." The horizontal grading, demonstrated by the persistently finer composition of the laminae of *2* as compared to *1*, is only slight over a distance of 1 meter. Compared to the small-scale experiments with 3 liters of suspension, the horizontal grading with forty times this amount of suspension is already stretched to at least ten times the distance. In nature the stretching should go so far as to render horizontal grading imperceptible.

Another interesting point is the concentration in *3* of coarse grains in the lower part of the bed, which is not shown in the analysis of the sand. But it should be borne in mind that the largest grains will be dropped first at the point where sample *3* was taken. This sample represents only 0.2 per cent of the whole suspension. So if there are only very few coarse grains in the suspension, these grains will nevertheless bulk large in the graph.

The data concerning *3* in figure 5 are of some importance because they show that grading can be produced by a flow containing no clay to speak of. But, as pointed out above, such a current can probably flow only a short distance.

Toward the far end of the ditch, the medium sand had gradually given out, and the grading was caused by very fine sand, silt, and clay. Part of this clay was deposited from the turbid water after it had come to rest. The beds show an almost perfect resemblance to varved deposits (pl. *2*, *B*), although the mechanism of deposition is probably different, as pointed out elsewhere.

Although it cannot be said how fine-grained was the last material dropped at each point along the ditch from the flow when it had passed, there was certainly some fine clay laid down on top from the turbid water standing in the ditch after the experiment. Two days were allowed for the water to clear and for the fine matter to become somewhat compacted. The topmost layer therefore was capped by a clay stratum at all points. Nevertheless, no clay deposit was found on the tops of layers over which a later flow had passed with a velocity of more than about 70 cm. per second, that is, for the first 4–5 meters beyond the slope. Evidently, the flows at this speed eroded the fine-grained and unconsolidated upper few millimeters of the preceding deposit in the few seconds they lasted. This also follows from the observation that the current B, II, although originally con-

taining no clay, eroded enough of the preceding deposit to lay down a thin lamina of clay all along the ditch. This added clay enabled a small amount of fine sand to be carried farther than was possible with A, I, a suspension without clay but flowing on a clean bottom. In nature, where there will have been a very much longer interval between flows, the material will have compacted more. On the other hand, the currents will have continued to flow much longer at each

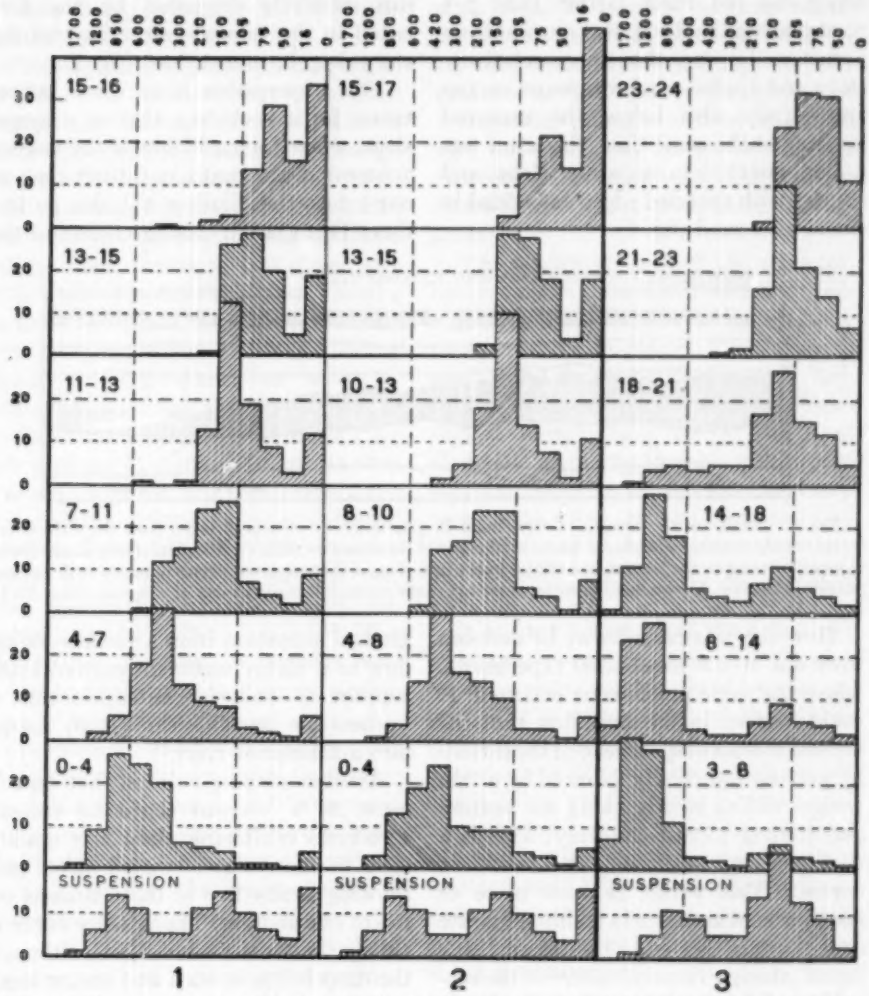


FIG. 5.—Histograms of graded beds; grain sizes in 1/1,000 mm., amounts in per cent. *1*, experiment A, flow II, 2½ meters from bottom of slope; *2*, same bed, 1 meter farther away; *3*, experiment A, flow I, at bottom of slope. The position of each sample in millimeters from bottom of bed is indicated. Note excellent grading, material becoming finer upward, and that *2* is slightly finer than *1*. Also note concentration in *3* of coarse grains which are so rare as not to have been found in analysis of sand sample used to construct composition of suspension.

spot, thus affording more opportunity for erosion.

As noted above, one of the flows (B, III) was too viscous to form a graded deposit. A relatively thick bed of uniform composition throughout was found, which did not reach farther than 7–8 meters along the ditch. But onward from 3 meters beyond the slope, relatively clean and graded sand was found on top and locally also below the unsorted lamina. At the same time, the latter was pulled apart into separate slabs and chunks with rounded edges embedded in the cleaner sand (fig. 6).

when they are considered to represent suddenly arrested stages in the gradual mixing of a thick suspension with water.

The graded sand continues beyond the unsorted material in a thin stratum reaching as far as the other layers. This was evidently deposited by the flow noted in the velocity curve beyond the steep drop in rate of flow. (fig. 4).

This observation is of some importance; for it is obvious that on a longer slope, where the rapid flow of the viscous suspension can continue farther than in our setup, the dilution will also go farther. This graphic demonstration of the

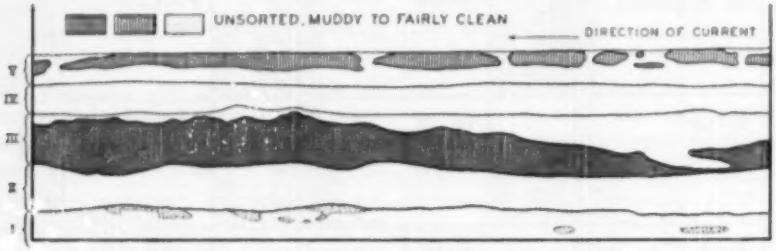


FIG. 6.—Section of the deposits in experiment B, 3 meters from bottom of slope. Note the slabs of unsorted muddy sand below or above much cleaner sand of the same flow (III, V). The small, fairly clean chunks of unsorted sand in the bottom bed are probably bits of sand that were not properly mixed with the suspension in the box. The undulating surfaces are not ripple marks but disturbances of irregular shape ($\times\frac{1}{3}$).

How this was caused may be deduced from one of the small-scale experiments where the same phenomena occurred. It could be seen in the aquarium that the normal turbulent spearhead of the turbidity current was closely followed by a thin wedge, sliding swiftly along the bottom and coming to rest suddenly. This is a type of slump and not a true turbulent current. This wedge behaves more or less as a solid and breaks up into separate slabs toward its end. Considering the liquid (though viscous) nature of the suspension, it is surprising to find that it can thus behave. However, it takes some time to mix oil with paint or water with a thick sludge of mud; so, after all, one can understand the features mentioned

gradual alteration from a viscous sliding flow to a highly turbulent current lends support to Bailey's contention that a "submarine landslide can readily merge into a submarine river."

Another striking point is that experiment B, V, showed the same curious structures combining clean and muddy sand in chunks and twisted flakes and showing locally two or three laminae on top of one another. The velocity curve is closely similar to that of B, III, although the drop is less sudden and occurs some 3 meters farther away. The reason for this behavior is not clear, considering the lower clay content, but it may be due to the smaller initial slope due to previous deposition. However that may be, it is

interesting to note that the sudden drop in velocity and the curious type of deposit appear to go together.

Slump- and other structures.—Normal ripple marks were not formed by any of the artificial flows. But the upper surface of some beds, especially B, II, locally showed irregular undulations.

Cross-bedding was conspicuous by its absence. It is believed that both these features may perhaps develop if a more dilute suspension can continue to flow for some time over the same spot.

In experiment A, after a few beds had been built up, the bottom had acquired a slight slope. Owing to this slope, part of the mass slid a small distance, causing marked folds and disturbances in the beds (pls. 3, *A*, and 4, *A*, *B*). The sliding appears to have taken place before or during the deposition of the last stratum. No ruptures were seen, but these may have occurred in parts of the strata that were not examined. The slump structures were mainly restricted to the near end of the ditch, but locally they were also discovered at the far end in the silts and clays, presumably by slip toward the central axis of the ditch (pl. 2, *B*). The combination of slump structures and graded bedding forms a striking parallel to many natural occurrences.

The slope at which the slumping occurred was remarkably slight. Certainly, the surface slope cannot have been more than 1:100. There is no evidence to indicate whether the sliding was due only to gravity or whether the frictional drag of a later flow helped to release it.

GRADED BEDS IN THE APENNINES AND OTHER REGIONS

THE "MACIGNO"

In the northern Apennines, the bulk of the autochthonous series is made up of alternating sandstone and mudstone of Oligocene age, known to Italian geologists as "macigno." The Middle Miocene "formazione marnoso-arenacea Romagnola," which is very similar, is extensively exposed along the Adriatic slope of the same area. Incomplete sections render thicknesses uncertain, but they certainly are very considerable and may be estimated at around 2,000 meters each.

Only the macigno will be described here, but practically all that will be said concerning it is equally applicable to the "formazione marnosa-arenacea Romagnola."

The sandstone beds of the macigno, which vary in thickness from a few millimeters to upward of 8 meters, are usually coarse or medium grained. They are well graded and show no sign of current bedding or lenticularity. Their lateral variation is inappreciable in all directions along the distances through which they may be followed in outcrop: in many cases up to 5 km., and in a few cases up to nearly 10. The mudstones are usually fine grained, and locally they contain some silt.

It is easy to see that these features when taken together, clash with the currently accepted mechanism of sedimentation. The transportation of the coarse grains of the macigno sandstones would need fairly swift currents, which would unavoidably produce false bedding and lenticularity and would inhibit grading. Moreover, it is difficult to conceive that currents could bring about uniform deposition over the extensive areas implied by the inappreciable lateral variability of the sandstone beds.

Some years ago these simple considerations led one of us (Migliorini, 1943) to suggest that the macigno and other similar formations of the northern Apennines had not been deposited in the ordi-

nary manner but had settled out of clouds of turbid water. With heavy sedimentation of largely arenaceous material in littoral waters, the seaward slope of the newly deposited detritus will increase progressively until the maximum angle of repose is reached. At this point, submarine slumping could be initiated by relatively insignificant agents, such as deep-reaching wave action during heavy storms or an especially abundant supply of sediment during floods. Earthquakes might also provide the trigger action but would probably not occur so frequently.

The seaward slope on which the slumps arose would be made up largely of loose arenaceous material (the argillaceous products of erosion would be carried farther out to sea), which, on slumping, would promptly be churned up into a turbid mass of higher density than that of the surrounding clear water. This mass would then flow down the submarine slopes until it reached the bottom, where it would spread out and deposit its suspended load as a graded sediment.[6] In the intervals between the arrivals of the dense turbid clouds, normal sedimentation would continue and would be of a nature determined by the distance from the shore of the areas at which the clouds spread out.

Clearly, this mechanism would give rise to graded sandstone beds of appreciably uniform thickness and composition over the whole of the depression in which the dense turbid clouds spread out. Alternating with the sandstone beds there would be normally deposited fine-grained sediments. The puzzling features of the macigno would thus be accounted for.

[6] It may be remarked incidentally that the rapid displacement of the large bodies of water involved by the mechanism suggested should give rise to tsunamic waves without any earthquake necessarily having taken place.

After the results experimentally arrived at by one of us (Kuenen) came to be known, other features were described which supported deposition of the macigno in the manner suggested (Migliorini, 1949c). Only some of them will be briefly outlined here.

1. The under surfaces of the macigno sandstone beds are in many cases literally covered by very clear counterimpressions of wormlike tracks left by some organism on the top surface of the underlying mudstones (pl. 5, *A*). That the mud was quite soft when the sandy material was deposited over it is borne out by the fact that the coarser grains of the latter left their imprints on its surface. The basal zones of the sandstone beds contain coarse grains up to 2 mm. across, which, under ordinary circumstances, could have been transported only by fairly swift currents which would necessarily have caused some erosion on the soft muddy bottom over which they flowed; the clear-cut worm tracks show no sign of any such action.

Over the flat floor of a submarine depression, on the other hand, an overloaded turbidity current could advance quite slowly, so that its erosive action on the floor over which it spread out would be negligible.

2. The sandstone beds of the macigno are graded in the sense that the sizes of their largest grains decrease from the base upward, but at all heights smaller grains of the entire range contained in the whole bed are found mixed with the larger ones, even the finest appearing from the very base.

Obviously, sedimentation by bottom currents could not bring about this form of grading because the size of the grains laid down at any instant would be limited to a very narrow range grouped about the maximum size transportable by the

current velocity prevailing at the time of deposition. Sedimentation from a slowly moving turbid cloud touching the bottom would, on the other hand, produce the type of grading described.

3. Flakes of shaly mudstone are, in places, more or less irregularly disseminated in otherwise perfectly regular and well-graded macigno sandstone beds (pl. 5, *D*). The mudstones of these flakes are apparently identical with those intercalated between the sandstones themselves. The flakes range in size from minute scales to large splinters around 5 cm. and in some cases over 10 cm. across. They are nearly always very sharp edged and quite thin, in some cases no thicker than a piece of paper. In most places they lie fairly parallel to the bedding planes, but locally are quite steeply tilted and even vertical. As a rule, the flakes are not concentrated near the base of the beds but commonly lie in the middle or upper sections.

The flakes cannot have been dragged along by bottom currents. These would have had to be very swift to move such large bodies, and with such swift currents the sand grains among which the flakes are embedded could not have settled out. Moreover, such thin flakes of soft rock would unavoidably have been broken and rounded if transported in this manner. Hence they must have been borne in suspension by a turbid flow of high density. This mechanism obviously tallies with the one suggested for the sedimentation of the macigno.

The shape of the flakes would favor their suspension—just as occurs on a smaller scale with mica—and they would only settle out when the turbulence of the suspension abated or its density diminished sufficiently. Evidently these conditions would be met somewhat to the rear of the front of the advancing turbid flow and after the larger sand grains had already been deposited; this would explain why the mudstone flakes are not usually concentrated in the basal zones of the sandstone beds, as might perhaps be expected.

The apparent identity of the mudstones of the flakes with those intercalated in the macigno sandstones is also readily explained by the mechanism suggested. Unlike arenaceous material, which consolidates mainly by the cementing action of circulating fluids, muddy deposits may be consolidated to a considerable extent by mere compaction under a small load soon after deposition (Ortenblad, 1930; Jones, 1939). Accordingly, during the slumping on the continental slope which started the turbid flows, fragments of consolidated mudstones could readily be picked up and borne forward in suspension along with the finer material.

THE BRECCIOLAS

In the northern half of the Apennine mountain arc, the macigno rests conformably upon very fine-grained, dark-colored shales, locally known as "scaglia,"[7] which form a comprehensive series including at least part of the Upper Cretaceous, all the Eocene, and extending into the Oligocene. In the uppermost section of the scaglia, sedimentary limestone breccias make their appearance, in many places containing abundant Paleogene macro-Foraminifera. These breccias are of a very distinctive lithological type and will be referred to as "brecciolas,"[8]

[7] The use of this term (not to be confused with "argilla scagliosa") is not strictly appropriate in conjunction with this shaly complex. True scaglia, in the accepted usage of Alpine and Apennine geologists, although it is not very dissimilar and occupies the same stratigraphic position, is a thicker and far more calcareous formation.

[8] In Italian, "brecciola" is the diminutive of "breccia."

which is the term by which they are known to Italian geologists.

In discussing the mechanism by which the brecciolas were laid down, attention should be drawn to the following features, which will be repeatedly referred to: (1) Both in individual beds and from one bed to another, the fragments making up the rock may range in size from several centimeters (10 or more) to fractions of a millimeter. (2) The brecciola beds, which range in thickness from a few centimeters to over 5 meters, are well graded and in most cases show no sign of current bedding. (3) The rock fragments making up the brecciolas are practically all angular and sharp edged (pl. 5, *C*). (4) Unlike the macigno, the brecciolas do not outcrop extensively. It may be inferred, however, that their beds are not continuous over great distances; for, although their lateral variation in lithology and thickness is not pronounced enough to show in individual outcrops (pl. 4, *C*), the total thickness of the brecciola complex may vary widely between outcrops lying at 10 km. or so apart. (5) The brecciola beds alternate with very fine-grained, dark-colored shales which are indistinguishable from the scaglia and have the appearance of a pelagic deposit. (6) The rock fragments that make up the brecciolas are predominantly limestones and subordinately crystalline schists. Many of the limestones are organic (pl. 5, *C*). (7) Very commonly the brecciolas are seen to contain abundant and varied macro-foraminiferal faunas. These have been extensively studied from many localities, and the conclusion may now be confidently drawn that they are made up of forms derived from older formations like the rock fragments with which they are associated (Migliorini, 1944*b*, with bibliography).

It has recently been pointed out (Migliorini, 1949*a*) that, if one attempts to account for the deposition of the brecciolas by ordinary sedimentary processes, it will be found that some of the above features clash irreconcilably with one another.

A. Coarse and very coarse detritus like that which in many cases makes up the brecciolas (see 1) could only have been deposited fairly close inshore by swift currents, which would of necessity have given rise to some form of current bedding, of which there is no trace (see 2).

B. It is hard to conceive that the grading of each brecciola bed (see 2) reflects a sudden increase followed by a gradual decline of erosion in an adjoining land mass. This would, in its turn, imply corresponding crustal uplifts and subsidences which would have had to be very numerous to account for the repeated lithologic alternations (see 5). It seems scarcely decent to suppose that our Mother Earth could ever have indulged in such skittish behavior. Moreover, the soft rock fragments making up the brecciolas would have been promptly rounded instead of sharp edged as they are, if they had been brought to the coast by rivers (see 3). One is thus induced to ascribe the grading of the brecciolas to sedimentation from a suspension of high density, and this process could not have taken place in the agitated littoral waters generally associated with such coarse sediments.

C. The shales intercalated in the brecciola beds (see 5) are obviously a pelagic type of deposit, whereas the brecciolas themselves would be littoral according to the commonly accepted laws of sedimentation. Therefore, the repeated alternation of these two lithologic types would again necessarily imply the inadmissible crustal palsy mentioned previously, but now in the region of deposition.

If it be supposed that the brecciolas were deposited by turbidity currents, the difficulties connected with the interpretation of their origin vanish. Briefly, turbidity currents, starting from shallow shelf waters and not necessarily of high density from the outset, are supposed to have flowed down the continental slope, eroding it and becoming loaded with the resulting detritus. With decrease of gradient at the foot of the slope, they would have lost velocity and turbulence and would have deposited their loads, starting with the largest fragments and continuing until the finer ones had settled out.

The mechanism would have been similar to the one which resulted in the sedimentation of the macigno, but in the latter case sedimentation would have taken place from a mass of turbid water, which, having spread out in a depression of the sea floor, flowed comparatively slowly, whereas the turbid currents responsible for the brecciolas would have had considerable, though progressively decreasing, velocity and would in most cases have been of smaller volume. In the first case only vertical grading would be appreciable, and lateral variation would be scarcely noticeable; in the second, there would be both vertical and horizontal grading, and lateral variation would be noticeable. This tallies satisfactorily with observed data.

THE APENNINE OROGENY AND THE SEDIMENTATION OF THE BRECCIOLAS AND THE MACIGNO

An attempt will be made at this point to see how the sedimentary mechanisms suggested fit in with the geology and the geological history of the Apennines. For readers who are not conversant with Apennine geology, a few fundamental aspects of the regional geology will be stated.

The Apennines, with their continuation in Sicily, form an irregular arc, with its convex side facing the Po Valley, the Adriatic, and the Ionian Sea. The local mountain-building, which advanced from the inside to the outside of the arc (Merla, 1949), began in the Upper Cretaceous and continued intermittently until the Quaternary. In the northern part of the arc two main phases may be distinguished in the orogenic cycle: the Paleoapennine in the Upper Cretaceous, and the Etruscan from the Oligocene to the Quaternary (Migliorini, 1945). The mountain ranges formed in the Paleoapennine phase have since foundered in the Tyrrhenian Sea, but their remains abound in vast disconnected autochthonous masses spread over the more recent formations of the outer and surviving belt of the mountain arc formed in the Etruscan phase.

During the long orogenic lull between the Paleoapennine and the Etruscan phases, a continental shelf should have begun to form off the inner shore of the geocynclinal trough, along the outer side of the Paleoapennine arc. The building-up of this shelf need not have reached an advanced stage, and older pre-orogenic formations down to the crystalline basement may still have outcropped here and there on its submerged surface. It would thus have resembled the present-day shelf off the coast of California (Daly, 1942, p. 154; Kuenen, 1947, p. 65; Shepard, 1948).

The epicontinental formations of this shelf would have been largely organic. Late Cretaceous and Eocene clastic sediments, both autochthonous and allochthonous, are scarce in the Apennines, so that sedimentation during this period must have been slight. This, of course, is

to be expected with the sluggish erosion accompanying prolonged orogenic inactivity. With such small amounts of detrital material reaching the coast, littoral waters would have been clear and conditions favorable for lime-secreting organisms.

A small part of the organic limestones, of Upper Cretaceous age, may have been formed by reef-builders (rudistids), but those of Eocene age, which would have formed the bulk of the shelf, probably consisted of organic detritus and calcareous mud because all the Eocene organic rocks found in the Apennine foreland are of this type. No true reef limestones are known anywhere in the Paleogene of the Apennine region.

Up to the very end of the orogenic lull, most of the sediments of the continental shelf, with the possible exception of the rudistid limestones, could not have been completely consolidated. No uplift had occurred; therefore, the calcareous detritus could not have been firmly cemented by circulating waters. It may be remarked that this is the condition that is believed by some to prevail to the present day in the sediments of the continental shelf of the North American Atlantic coast down to a depth of nearly 4,000 meters (Daly, 1942, p. 109).

During the orogenic lull and while the continental shelf was being built up, the scaglia was being deposited farther offshore by scanty and very fine-grained sedimentation in the area where the ranges of the northern Apennines now stand.

It is only natural to suppose that the Etruscan orogenic phase proper was heralded by a milder crustal deformation, during which the continental shelf was raised enough to bring its surface within reach of energetic wave action. This would, in turn, produce turbid currents which would flow first over the shelf and then, with increased velocity, down the continental slope, where they would erode channels. Except for the cause of the shallowing of the water on the shelf, the mechanism is identical with the one that has been suggested for explaining the origin of submarine canyons (Kuenen, 1947, with bibliography).

As already mentioned, the speed of the turbid flows would be quickly checked at the foot of the continental slope by the easing of the gradient, and the coarser suspended detritus would be dropped as a horizontally and vertically graded sediment, thus forming the brecciolas. The deposited material would not be washed clean like an ordinary current-laid sediment: as already remarked, the high viscosity of turbidity currents practically eliminates saltation, so that any finer particles that happened to reach the bottom with the coarser ones would tend to remain lodged among the latter, along with entrapped turbid water.

After having deposited their coarser material, the turbid flows, carrying only fine and very fine particles, could continue their courses for great distances down even very gentle slopes and would come to rest only when they lodged in a submarine depression capable of containing them. Here the remaining load of finest particles would slowly settle out in the manner suggested for the macigno.

At the foot of the continental slope each turbid flow would spread out and be sharply checked in speed, so that its sediments would assume the shape of a flat alluvial fan issuing from the outlet of the channel down which it had flowed. Where the channels were closely spaced, the individual fans would overlap laterally, whereas no brecciolas would be deposited between widely spaced channels. This tallies well with the observed dis-

continuity of the brecciola complexes (see 4, p. 112). The whole process outlined is illustrated schematically in block diagrams *B* and *C*, figure 7.

It should be borne in mind that the deposition of each brecciola bed would be a very swift process and that, whereas the whole brecciola sequence would have been determined by a slight uprise preceding the Etruscan orogenic phase, the individual turbid flows would have been initiated by occasional heavy storms or other irregular unexceptional agents. During the intervals between turbid flows, the type of sedimentation normal to the areas in which the brecciolas were deposited would continue, and thus the scaglia intercalations would be deposited.

According to Shepard and Emery (1941), certain low delta-like bulges, occurring at the lower ends of some of the canyons cut into the continental slope off the California coast, appear to correspond satisfactorily with the brecciola fans that would be formed in the manner suggested. Kuenen (1947, p. 60), in fact, actually did interpret these features as the result either of sedimentation effected by turbidity currents in a manner practically identical to the one suggested above or of slumping.

It may also be remarked that the suggested mechanism affords a satisfactory explanation of a feature of the derived macro-foraminiferal faunas that is otherwise very perplexing: the great abundance of Eocene *Nummulites* and the rarity of Oligocene *Lepidocyclinas* in the brecciolas of Tuscany (Migliorini, 1944, p. 79). Clearly, the sediments of the continental shelf built up between the Paleoapennine and the Etruscan orogenic phases would have been mainly of Eocene age because this period comprises the greater part of the time interval involved. It is only natural, therefore, that Eocene rocks and Eocene fossils should predominate in the derived materials making up the brecciolas, although the deposition of the latter occurred mainly during the Oligocene.

The transition from the brecciolas to the macigno and, after that, the macigno itself will be considered in conjunction with the geological history of the Apennines. In sections that have not been too disturbed tectonically, the contact between the brecciolas and the macigno is perfectly regular and concordant. In places the two formations actually merge into each other by repeated alternations. But, notwithstanding this perfect sedimentary continuity, typical macigno sandstones always make a sudden first appearance in a brecciola and shale alternation. From this evidence the following deductions may be made: (1) At a given moment during the deposition of the brecciola formation there was a sudden influx of another type of sediment, consisting mainly of arenaceous material. The new type of sediment, which gave rise to the deposition of the macigno, obviously resulted from the erosion of a different source area than that from which the material of the underlying complex was drawn. (2) The conditions determining the sedimentation of the brecciola formation persisted for some time after the beginning of the macigno sedimentation. (3) The changeover to macigno sedimentation was not accompanied by any appreciable tectonic disturbance in the area of sedimentation.

An attempt will now be made to fit the above deducations into the geological history of the northern Apennines. At some time during the Oligocene, the Etruscan orogenic phase proper began. This would have immediately followed the hypothetical preliminary uplift that had brought about the deposition of the

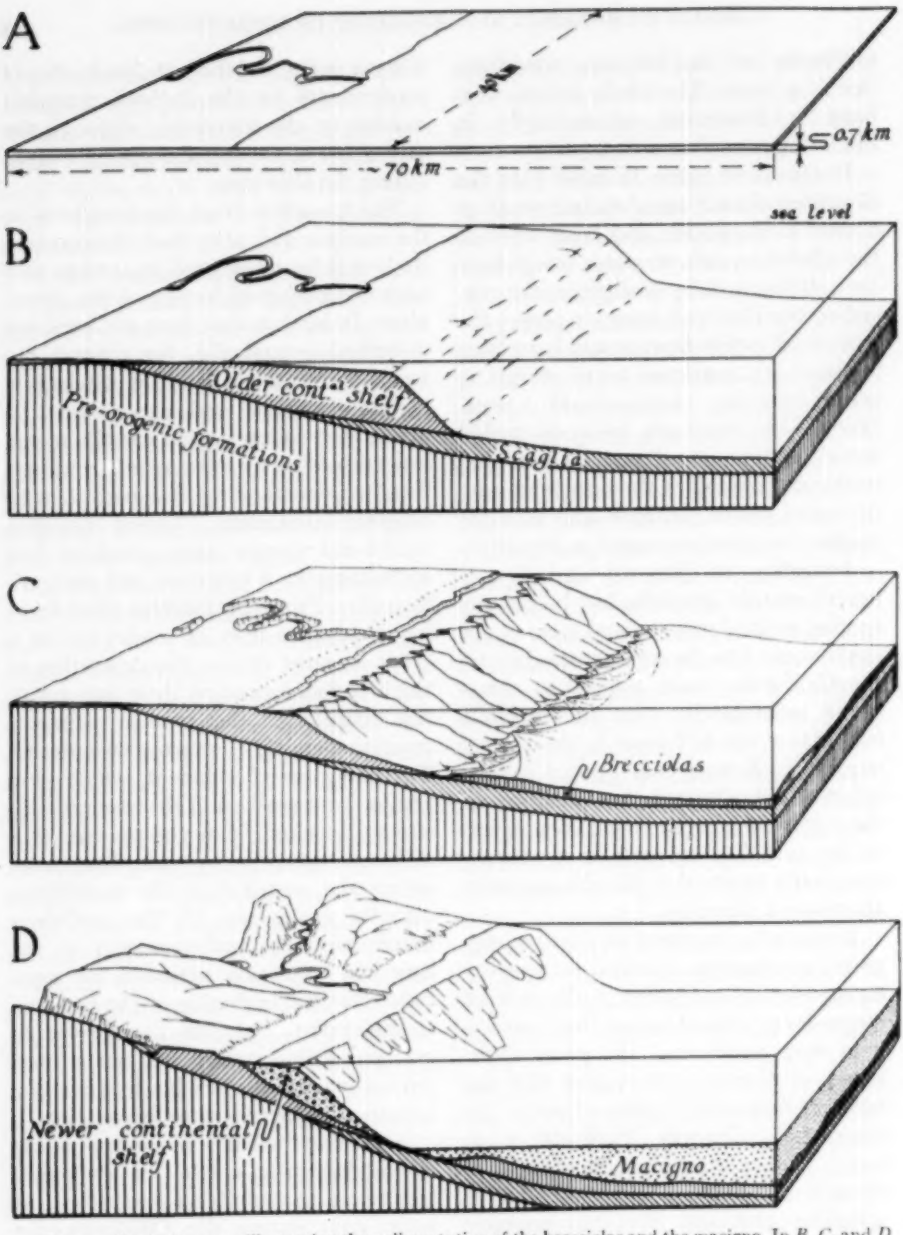


FIG. 7.—Block diagrams illustrating the sedimentation of the brecciolas and the macigno. In *B*, *C*, and *D* the vertical scale is some twenty times the horizontal. *A* is drawn to true scale, with indicative dimensions. *B* shows the phase immediately preceding the sedimentation of the brecciolas. No irregularities are shown along the base of the continental shelf, to avoid unduly complicating the drawing. In this phase, terrigenous

(Legend continued on facing page)

brecciolas in the manner suggested. The mainland behind the continental shelf from which the brecciola materials were derived was rejuvenated, and the ensuing erosion provided the sediments of the macigno, the composition of which indicates that it was derived mainly from igneous and metamorphic rocks. Erosion must have been vigorous and widespread, to judge from the great thickness, wide distribution, and lithologic uniformity of the macigno. Moreover, the sea floor would have descended fairly steeply from quite close to the coast where the eroded materials arrived, because the continental shelf formed during the preceding orogenic lull would have been in large part demolished by submarine erosion by this time. Thus the seaward slope of the fringe of largely arenaceous materials brought down by rivers and distributed along the coast by wave action would soon have attained its maximum angle of repose and would then have started to slump, giving rise to dense clouds of turbid water that flowed down the slope of the sea floor and deposited the macigno farther out at sea. In short, conditions would have favored the sedimentary process suggested to explain the deposition of the macigno.

When the sedimentation of the macigno began, the remains of the shelf built up during the preceding orogenic lull may not have been buried in all places by the newly arrived sandy deposits along the coast. Where they were still exposed, they would have been subject, as before, to submarine erosion, thus providing the materials for the brecciolas intercalated in the basal zone of the macigno.

The absence of any tectonic disturbance in the area of sedimentation during the transition from the brecciolas to the macigno is in harmony with what is known of the geological history of the northern Apennines. Because the belt undergoing orogenic deformation migrated eastward from the inside to the outside of the mountain arc, it follows that when the Etruscan phase began inland and brought about the rejuvenation that resulted in macigno deposition, it would have had no effect in the more easterly districts, where sedimentation from turbid clouds was taking place.

The intermittent slumping would have retarded, but not prevented, the advance

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sedimentation would be scanty because of the senile topography of the mainland. The sea would be slowly retreating because of the growth of the continental terrace, mainly by organic accretion.

In *C* the littoral zone has been raised some 30 meters. As described in the text, this has brought about the erosion of the continental shelf by waves and of the slope by turbidity currents and the deposition of brecciola fans at the foot of the slope. In the phase shown, the destruction of the terrace is well under way but has not yet reached its maximum. Following the uplift, the sea would first have retreated and then have advanced by erosion consequent on the progressive destruction of the shelf.

In *D* the mainland has been uplifted by orogenic deformation, and its rejuvenation is in progress. The strange topography is merely an effect of the exaggerated vertical scale: the greatest elevation represented would be only some 270 meters and, if drawn to true scale, would be barely perceptible. Before orogenic deformation, the demolition of the continental terrace and the deposition of the brecciola fans had reached a more advanced stage than the one represented in diagram *C*. The new continental terrace, which has already buried the submerged remains of the older one, is growing, a littoral plain is forming behind it, and the sea is retreating. Slumping along the unstable slope is bringing about the deposition of the macigno by turbidity currents in the manner described in the text.

It should be remarked that the upper surface of the brecciola fans, which were deposited by swiftly moving suspensions, slopes out to sea and is uneven, whereas that of the macigno, which was deposited by a suspension that spread out on the bottom of the sea floor, is practically horizontal and even. Consequently, the brecciola formation thins out, whereas the macigno thickens in a seaward direction.

of the seaward slope of the sandy coastal belt, and in time a new continental terrace would have formed, burying the older one from which the brecciola material had been derived. The new terrace, consisting mainly of sandy material, would have exhibited a marked lithologic contrast to the older one, in which organic and pelitic accumulations predominated. As a result of this lithologic difference, there would have been a marked difference in consolidation as well; for, whereas the muds of the earlier terrace could have been promptly consolidated by load alone, consolidation of the sandy material of the newer terrace would have required the cementing action of circulating waters, and this was impossible as long as the deposits remained submerged. Moreover, the time factor favored the consolidation of the older terrace, which was built up at a slower rate than the newer one

This explains why submarine erosion could cut channels in the older terrace to provide the coarse material of the brecciolas, and not on the unstable slope of the loose material of the newer terrace, where any factor tending to disturb equilibrium would have initiated the broad slumps and therefore the dense turbid clouds charged mainly with sand.

A comparison of the material of gravel grade in the brecciolas and in the macigno bears out the mechanisms suggested for the sedimentation of the two formations. Except for the mudstone flakes that have been mentioned earlier, the coarser fragments occurring in the macigno are in most cases well rounded and consist mainly of hard igneous and metamorphic rocks, so that the soft, sharp-edged, and dominantly calcareous fragments of the same size in the brecciolas are truly striking. For shapes of the fragments in the two deposits, compare plate 5, *B*, *C*, and *E*. Clearly, this contrast is just what one would expect: it would follow that the detritus of the two formations would not only have been derived from different areas but also have been transported in a different manner. Material of the brecciolas would have been carried in suspension by a turbid current over a comparatively short distance and would have undergone little or no wear, whereas the material of the macigno would first have been transported overland by river action, becoming well rounded in the process, and then have been borne in suspension by submarine turbidity currents. The conditions determining the deposition of the macigno are schematically shown in block diagram *D*, figure 7.

POSSIBLE GENERALIZATIONS

The mechanism of sedimentation of the brecciolas has been suggested specifically for the autochthonous series of the northern part of the Apennine arc, but the paleogeographical conditions determining it are in no way exceptional and could easily have occurred at the beginning of orogenic phases in many other deep geosynclines.

Outside the northern Apennines, one of us (Migliorini) has come across formations identical with the brecciolas in southern Italy, in the Dodecannese, in Asia Minor, in Cyprus, and in the Carpathians. No doubt a much more complete list of occurrences might be gleaned from geological literature.

Similar arguments are applicable to the macigno. The conditions bringing about the mechanism suggested for the deposition of these beds might exist temporarily in many orogenic belts, and complexes similar to the macigno are actually found in many cases of synorogenic formations. Outside the northern

Apennines, one of us (Migliorini) has noted such complexes in the southern Apennines and in Sicily and in the Tertiary sediments of Istria, the Carpathians, the Dodecannese, and Cyprus. An Upper Cretaceous sandstone and shale alternation found in the Apennine allochthonous area (where it is known as "pietraforte") and on the island of Rhodes has practically identical features. A Permian or Carboniferous formation occurring north of Macri in western Asia Minor and the probably Devonian Gramscatho series of the Cornish Killas are also very similar.

It should be pointed out that, if the sedimentary hypotheses suggested in this paper prove to be correct, certain currently held ideas on sedimentation, especially those involving graded beds, would have to be radically revised, along with their paleogeographical implications, as has already been remarked (Migliorini, 1949*a*). Moreover, he deems it advisable to introduce a major distinction between *directly deposited* and *redeposited* clastic formations. The first class would include formations of which the bulk of the material had been transported directly from the point at which it reached the body of water (sea or lake) in which it was to be deposited to the point at which it was actually laid down. The second class would include formations of which the bulk of the material had been transported from the point of arrival to the point of deposition in two or more stages and had, in the intervening pause or pauses, entered into the composition of a true sediment.

THE CAUSE OF GRADED BEDDING

DIFFICULTIES IN EXPLAINING MANY TYPES OF GRADED BEDDING

As pointed out above, some types of graded bedding can be explained by the settling of fine particles in water of some depth. Except for some volcanic beds, only fine-grained deposits can be accounted for by this process because winds and surface currents can transport only fine sand, silt, or lutite in suspension. Varved series are probably formed in this manner from silt and lutite carried out along the surfaces of glacial lakes. The grading of varves is due partly to the long time required for the lutite to settle in cold waters and partly to the gradual decrease in grain size after the maximum influx at the time of the spring meltwaters. The fine lamination sometimes observed in thick varves may be ascribed to variations in discharge of the meltwater rivers during a single season. Flocculation of the lutite causes it to settle as swiftly in sea water as the silt does. Hence, varves are rarely of marine origin (Fraser, 1929).

Some medium and coarse beds are graded because the supply of material shows variations in grain size. If the material is carried by a river, the beds should be of limited extent and irregular consistency, and cross-bedding, scour, and inverted grading should be normal features of the deposit. In other cases the accumulation of the deposit automatically lessens the competency of the supplying agent. Thus some shingle ridges are graded because the waves cannot fling the coarser pebbles as far up the sloping front as they can the smaller ones. Deltas and alluvial fans may by their growth appreciably lessen the slopes of their streams and thus result in grading.

There remain several important types of graded beds for which a different mechanism of deposition must be sought. One must account for beds containing chunks of clay, especially when these chunks are flaky or angular and occur

over so wide an area that a considerable distance of transport is indicated. Normal transport by rolling should have caused rounding and rapid distintegration of clay fragments. Also, there are coarse graded deposits of pebble size or larger laid down in an environment where swift currents are necessarily excluded, such as the present deep-sea, geosynclinal series in which current bedding, ripple marks, signs of local erosion, shallow-water benthonic organisms, and other evidences of shallow water are lacking. Or the coarse beds may alternate with fine-grained deposits containing deep-water benthonic organisms (certain Foraminifera).

In typical thick series of graded beds the areal extent of each layer tends to be so large that it cannot represent the accumulation on a river bed. In many cases marine fossils also prove that deposition took place on the sea floor. Here the only currents of sufficient strength to move coarse sand are of tidal nature. These do not attain great velocities over wide areas but only in restricted channels. Where they do occur, scour tends to prevail, and ripple marks are common.

The volume of the thicker beds is too great to be accounted for by deposition in one season. Hence the material must have first accumulated elsewhere over a long period, then been transported and finally redeposited in a graded bed at the present site. This transportation must have occurred without erosion of the former unconsolidated and fine-grained top layer of the preceding bed. Yet coarse sand and even pebbles are found at the bottom of coarser members. These larger particles were not rolled along the bottom but were carried in suspension: otherwise cross-bedding would inevitably have developed.

POSSIBLE EXPLANATIONS FOR THESE CASES

Four possible explanations must be investigated.

1. *Tsunamic waves.*—"Tidal" waves at times dash onto a beach and the land farther inland. In returning, they carry a vast amount of eroded sediment out to sea. Although moderately coarse sand might be thus spread out in the surface waters, it could not be carried far from shore. Manifestly, coarse sand and pebbles cannot be thus transported. Moreover, the oscillation is generally repeated several times, and the grading of the resulting deposit should be multiple. Great irregularity in grain size, thickness, and composition of the graded bed over short distances should result from variations in amount and composition of sediment available in the coastal area invaded by the wave. For all these reasons, few of the graded beds under discussion could be accounted for by direct action of tidal waves.

2. *Slumping.*—A wide range of intraformational structures are ascribed to slumping (also termed "slip bedding" or "subaqueous sliding"). In these cases the original bedding, although much distorted or even broken up, is still largely retained, and most of the sedimentary particles have remained in contact with their original neighbors. Hence the slump movement cannot have caused grading in an ungraded deposit. On the contrary, it can only have greatly obscured any grading previously developed.

3. *Mudflows.*—Probably no sharp distinction can be made between a slump and a mudflow. In the latter the material is in the thixotropic state. It is disintegrated during the more or less plastic flow and thoroughly mixed. In fact, according to Blackwelder (1928, p. 465), mudflow deposits (fanglomerates) are

characterized by the absence of sorting: "The deposit as a whole consists of beds of unassorted and unstratified earthy material, a heterogeneous mixture of particles of all sizes, which in that respect resembles glacial till and some volcanic agglomerates." This was confirmed experimentally for subaqueous mud flows, as pointed out above. These mudflow deposits tend to show an original slope of 4°–6°. An eyewitness described how "immense masses of rock, many of which must have weighed several tons, were dancing along on the surface, apparently as light as corks." After the flow comes to rest, these large blocks remain protruding from the surface and do not sink down into the sludge. However, it is not inconceivable that some mudflows, especially if occurring on the sea floor, are less viscous and show some grading in the coarse material that they have carried along. But, on account of the high internal friction of a sludge in this state, finer sand grains and silt can certainly not separate out and form a graded deposit. For this reason, any bed in which the fine sand and silt grains are included in the grading cannot have been deposited by a mudflow. Therefore, none of the more extensive graded series can be explained by this mechanism.

4. *Turbidity currents of high density.*—These are of considerable importance and will be discussed in detail below.

THE NATURE OF TURBIDITY CURRENTS

There can exist no sharp distinction between turbidity currents of high and of low density, but the properties of the extremes are so different that it is convenient to distinguish between the two. At the lower limit the excess density of the turbid water is so small that it cannot overcome the inertia of the surrounding stagnant water. With slightly greater density, the suspension will flow along the bottom until it encounters water of equal density and then will spread out at this level over a wide area.

A typical turbidity current cannot arise unless the specific gravity of the flow is greater than that of the most dense stagnant water in the basin. The increase in density of ocean waters between surface and sea floor (apart from the influence of pressure) hardly anywhere exceeds 0.4 per cent. Hence the addition of 1 per cent by weight of sediment to surface waters would suffice to produce a suspension the density of which exceeded the density of the bottom water. This amount of suspended matter may serve to show the order of excess density of a dilute turbidity current. No more sand can be carried in suspension by this type of flow than in clear water.

At the other extreme the density is of the order of 2 with sand and clay or 1.5 with clay alone. At higher densities the mobility of the liquid suddenly decreases, and turbulent flow is no longer possible. Such material can form only a plastic mudflow. It can easily be shown that, in a moderately thick suspension of clay, sand grains cannot sink and even small pebbles remain in suspension for a considerable time. On dilution of the clay suspension by water, it is found that at a fairly well-defined stage the sand grains are suddenly released and sink swiftly to the bottom.[9] The density at which this occurs depends on the nature of the clay but is of the order of 1 part clay in 2–3 parts water by weight (density 1.2). Below this density, there is still a considerable retardation of the settling velocity of the sand, and moderate tur-

[9] Kuenen hopes to investigate this phenomenon more fully later.

bulence in flows of these densities will, therefore, still keep a large load of sand in suspension. It is not until a density of 1.06 (1 part clay in 10 parts water) is reached that sand is no longer carried in appreciable quantities in suspension at velocities of $\frac{1}{4}$–$\frac{1}{2}$ meter per second.

The term "turbidity current of high density" will be applied to currents of sufficient density to allow them to carry a large load of sand in suspension. The distinction between "high" and "low" density depends on the velocity and degree of turbulence, on the salinity of the water, on the nature of the lutite, and on the grain size of the sand.

At the bottom of the slope the flow will possess a large momentum and will flow for a great distance at a gradually decreasing velocity. If a counterslope is met, this decrease will be hastened, but the experiments show that the current can mount a long way before losing its momentum. All sand will be dropped, but the lutite may cause a slow return to the deeper parts of the basin.

GRADED BEDDING EXPLAINED BY TURBIDITY CURRENTS OF HIGH DENSITY

During decrease in velocity the flow must soon attain an overloaded condition, and settling will commence with the coarsest particles available. The carrying power of a turbidity current is truly astounding. Mudflows on land are known to carry huge blocks, which float even when the velocity is quite small. Thus it has been observed that where a sluggish "lahar" has passed, leaving trees erect, great boulders may remain suspended between forked branches several feet above ground. Blocks weighing hundreds of tons are known to have been transported by mudflows over distances of a dozen kilometers. Although turbidity currents are less dense, it may be assumed that they are also able to carry along large boulders on steep irregular surfaces when turbulence is great. Hence there is no difficulty in accounting for the inclusion of very large fragments in graded beds deposited by these currents.

The experiments show that, at a moderate rate of flow, pebbles can still be carried in suspension. For this reason we can account for the exceptional combination of features shown by some graded series: absence of erosion of previously deposited and unconsolidated lutite and transportation in suspension by the following current of very much coarser particles. One of the principal enigmas of graded bedding can thus be explained by turbidity currents of high density.

Another baffling feature—the astounding volume of the separate beds—is also accounted for because there is no obvious limit to the volume which a flow can attain. The sedimentary matter, accumulated over a long period of time on the upper slopes of a basin, can be raised and carried to the deeper areas in a sudden brief act.

We need no longer be puzzled by the transportation of clay fragments over long distances, because in a turbidity current even large chunks of this light material can be carried in suspension and need undergo no violent abrasion by rolling or concussion. The frequent occurrence of clay pellets in graded graywacke series is also readily understood.

The great areal extent of even thin graded beds is explained by the nature of the turbidity currents where they spread out on a flat basin floor. Whereas a river flows in a narrow channel, the turbidity current may fan out over a wide flat area. Because the flow drops its load from suspension and hardly moves it along the bottom afterward, no cross-bedding is developed.

On the large scale to be expected in nature, an individual bed should cover huge areas. In the small-scale experiments, 3 liters of suspension deposited its load along a channel in a wedge thinning to one-quarter of its thickness over a distance of ½ meter. In the large-scale flows, forty times this volume made a wedge only about twice as thick, but tapering to one-quarter of this amount over a distance of 15 meters. The important conclusion can be drawn that, *ceteris paribus*, increase of volume causes only slight thickening of the stratum but great increase in areal extent. If in nature the volume of suspension for each unit of frontal breadth were one hundred times that of the experiments, the bed should be only some 5 cm. thick and taper to one-quarter of this amount over a distance of the order of 1 km.

If grading were produced by the introduction of mixed sediment into surface waters of a deep basin, the largest grains should arrive at the bottom first without carrying much silt and lutite with them. Beds showing medium to coarse sand at the bottom should therefore be clean. It is found, however, that grading is typical of muddy sandstones and graywackes, in which, in many cases, more than half the material consists of interstitial lutite. We are, then, dealing not with closely packed sand containing lutite washed into the remaining pore space but with a deposit in which the large grains hardly touch. Evidently, deposition of fine matter has taken place simultaneously with that of the larger particles. This is exactly what might be expected if turbidity currents had formed the deposit, and the experiments confirm this deduction.

The commonly noted alternation of graded beds with slump structures and the graded nature of the sediment covering some slump sheets has been mentioned above. This combination shows that the bottom slope required for a turbidity current was available. The graded members may represent slumps diluted to turbidity currents, or such currents formed in some other way, but flowing down the same slope.

Less commonly, current bedding and grading are noted in the same series, although not in the same member of the series. Conceivably, the cross-bedding and ripple marks were produced by turbidity currents of lesser density and size which continued to flow leisurely past any given spot for a much longer time. This point is not yet clear, however.

It would appear, therefore, that all the exceptional and puzzling features of typical series of graded graywackes can be readily accounted for by the activity of turbidity currents of high density. Likewise, graded deposits of different nature, such as those described above from the Apennines, are likely to have been formed by the action of these subaqueous flows.

The explanation which Bramlette and Bradley (1940) offered to account for certain abnormal graded beds found in deep waters of the Atlantic can now be accepted with greater confidence. Kuenen has already suggested that many deep-sea sands (coarse deposits found in cores taken far from the coast) have originated by action of turbidity currents. This belief is here restated. The examination of deep-sea cores should in the future always include a search for graded deposits. Long cores taken in the vicinity of the lower end of submarine canyons should help to determine how far turbidity currents are and have been active in these valleys.

Under most circumstances, grading appears to afford a trustworthy feature by which one may decide whether a de-

posit was formed by a strongly turbulent turbidity current, as opposed to a viscous turbidity current, to a slump, or to normal settling. The study of the deposition in a basin, whether a present deep-sea trough or a fossil sedimentation basin, can therefore not be considered complete until the beds have been examined carefully for grading.

Several aspects should be especially noted in fossil basins: (1) the direction in which graded beds tend to thin out, (2) the direction in which the grain size tends to diminish, and (3) the occurrence, size, and shape of ripple marks. If these ripples are asymmetrical, the strike and direction of steepest slope should be noted at all exposures. These three properties together would indicate the source of the flows and the bottom configuration of the basin at the time of deposition.

Further important points are the thickness and grain size of the beds and the relation between these and areal extent. A comparison with the brecciola and macigno complexes should be made, to ascertain whether the same paleogeographic deductions are applicable.

THE DEVELOPMENT OF TURBIDITY CURRENTS IN NATURE

In conclusion, an attempt can be made to account for the development of turbidity currents of high density on the sea floor.

1. The mechanism considered most probable is the occurrence of a slump down a slope on which rapid sedimentation is taking place. Various intraformational deformations are attributed to this mechanism, and they have been shown to occur in recent deposits of the Black Sea on slopes no greater than 1° or 2°. Heim described slumping in Alpine lakes which is accompanied by turbidity currents, and O. T. Jones (1939) found graded deposits lying with a welded contact on slump sheets in Wales. As pointed out above, series of graded beds are in many cases associated with slip bedding. Either the entire slump or part of it might readily become diluted with water and thus change to a turbidity current of high density.

One step in this process was noted in the experiments, i.e., the dilution of a viscous turbidity current depositing ungraded sediment to a mobile suspension resulting in a graded stratum. The coarse graded deposits—the brecciolas—described from the Apennines are shown to have formed at the lower end of a submarine slope, probably as a kind of delta, whereas the finer ones—the macigno—may be considered to be of material which moved farther out into the basin and spread along the bottom. In the latter case the primary cause of the turbid flows may be the steepening of the slope by sedimentation until the maximum angle of repose is reached, the movement being initiated by a heavy storm, a flood supplying unusually abundant sediment, or an earthquake. The turbid flows which deposited the brecciolas, on the other hand, seem to have arisen as outlined in paragraph 3.

2. A mudflow on land which reaches the shore might continue along the bottom and change to a turbidity current. Little is known of the general conditions on land during the pre-Cambrian. It is conceivable that mudflows were generated on a more extended scale than at present because vegetation was presumably absent or meager. Thus the common occurrence of graded bedding in ancient deposits might be explained.

3. In shallow coastal regions, wave action caused by storms or tsunamis is known to churn up sediment. If sufficient density is attained, a turbidity current of

low density should start along the slope to deeper water. Provided that some erosion is effected by the current, the density could increase until high density is attained. The objection can be raised that in the experiments deposition took place and always exceeded the amount of erosion. But, with equal density and slope, enlargement of the scale produces swifter currents. It may well be that severe erosion does not start until velocities are developed greater than allowed by the small scale of the experiment. A fuller treatment of this aspect is given by Kuenen (1938, 1947).

There may be some doubt that turbidity currents of high density are ever developed in nature because they have never been observed to date. But it should be borne in mind that by their very nature they must always occur where they cannot be directly observed. Also, (1) they cannot be very common; (2) they flow below standing water and must soon descend to considerable depths below the surface; (3) they belong in turbid waters, otherwise there is not sufficient lutite available; and (4) either storms or earthquakes may initiate them when the observer is not in a favorable position to note any minor features that might be visible at the surface.

Rivers and streams tend to carry all available lutite in suspension straight to their mouths, and it cannot, therefore, accumulate in the beds. Hence, when the river is in flood, there is insufficient lutite available for increasing the density of the water to a value at which "high density" is attained. As far as the authors are aware, therefore, rivers never flow as high-density currents with a huge load of suspended sand.

One could imagine a turbidity current of high density developing along a river bed and flowing independently of the main body of water. However, the turbulence of the flowing river will soon disperse the silt and lutite throughout the whole channel and thus dilute the turbidity current to the point where coarser fractions are dropped. The same reasoning can be applied to tidal water. When the currents flow below *standing* water, there is much less mixing with overlying waters, which tend to drop acquired sediment back into the turbidity current. The experiments show that, even so, the dilution is a significant feature.

But now and again, especially in arid climates or on volcanic cones, a sudden cloudburst may develop mudflows (or lahars) along stream beds. These mudflows start to move as soon as the material is sufficiently lubricated to overcome friction. This is before they are diluted to the degree necessary for true turbulent flow. It is thought that they generally encounter drier material and cannot become diluted even if they do erode: they will continue to slide as a thick paste. Where they become mixed with clear water, dilution will generally go beyond the lower limit of high density. It must be rare for a mixture of the correct density to be developed.

On the sea floor, however, there is always a supply of water for moderate dilution, and in many cases sufficient lutite will be available. If dilution proceeds too far, the larger volume and greater mobility will cause more rapid flow, resulting in erosion and increase of the lowered density. The latter process is limited by increasing viscosity. Thus it may well be that on a submarine slope there is a strong tendency for the most effective combination of viscosity and density to develop. Admittedly, this is merely a suggested possibility.

The authors do not wish to maintain that they have presented irrefutable

proof of the occurrence of turbidity currents of high density on the sea floor and of the nature of their activity either by the experiments or by the deductive reasonings just presented. But they do believe that there is a considerable measure of probability in the hypothesis thus arrived at. It appears to account satisfactorily for the deposition of many graded series which had hitherto baffled all attempts at explanation, and this is perhaps the strongest argument in favor of the views here set forth.

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THE CAROLINA "BAYS"[1]

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ABSTRACT

Since the area of the bays is almost the entire area of the coastal plain in the two Carolinas, Johnson's complex theory of origin of the Carolina "bays" would violate the laws of conservation of energy, inasmuch as the area of assumed outflow of ground water would have to be the same as the area of intake. Johnson assumed implicitly that the coastal-plain bedrock (chiefly Cretaceous) dips down the slope of the coastal plain toward the southeast in the manner of a textbook illustration of coastal plains, whereas the dip is actually toward the south or slightly southwestward. Therefore, his assumed "spring vents" should have moved northward under his theory, instead of northwestward, as the shape of the bays would require. Other errors of fact and examples of illogical theory in Johnson's spring-vent hypothesis are pointed out.

Through a revision of the meteoritic theory it is shown that the supposed impact could have occurred at any one of several times in the geologic past.

INTRODUCTION

The publication by Professor D. W. Johnson of *The Origin of the Carolina Bays* (1942) and "Mysterious Craters of the Carolina Coast" (1940) constituted the most voluminous contribution ever made on this subject by one man. Other references are Melton and Schriever (1933) and Melton (1934*a*, *b*, and 1937*a*, *b*).[2] Although Johnson was an acknowledged leader in the field of geomorphology, certain features of these two contributions are open to criticism. The two will be treated as one, inasmuch as they appeared nearly simultaneously.

His theory of the origin of the bays has a fatal error which most geologists have probably recognized; but so wide was the publicity given to his ideas by the Sigma Xi publications that many scientists and laymen no doubt believe that the controversy is now settled.

WEAKNESSES OF THE JOHNSON THEORY[3]

Johnson's theory of origin embodied artesian, spring-sapping, solution, lacustrine, and eolian phases, each phase being discussed profusely in the two publications. The artesian and spring-sapping phase of his hypothesis—the nucleus of the entire theory—was based so largely on theoretical considerations and is so little clarified by concrete examples, maps, or cross sections that it is uncertain what the hypothesis really is. The "artesian phase" alone has, however, one fatal error. *The area of assumed outflow for the regional circulation of ground water must be the same as the area of intake*, inasmuch as the bays are distributed over nearly the entire area of the coastal plain in the two Carolinas. This assumed regional outflow would violate the laws of conservation of energy. It is not denied that there is a regional circulation of ground water in the buried and truncated Cretaceous and Eocene formations. But one of the simplest requirements for conducting water under a hydrostatic head through-

[1] Manuscript received November 21, 1949.

[2] The author is indebted to Professor William Schriever, of the University of Oklahoma, for assistance during the field study of 1932 and for helpful criticism of some of the author's publications on this subject.

[3] McCampbell (1944) has presented a lucid geological criticism of Johnson's "artesian" theory, to which the author is indebted.

out a region is that the area of intake shall be separate from the area of outlet.

In addition to the foregoing mechanical impossibility, the artesian and spring-sapping phase of Johnson's hypothesis has other errors (Ferguson, 1943*a*, *b*), some of which will be discussed below.

I. There are two aspects of artesian circulation of water in the region where the two Carolinas border on each other and where the bays are best developed which are not clearly separated by Johnson. First, there exists a relatively deep regional circulation in rocks mainly of Cretaceous and Eocene age. Second, there is a shallow and local circulation in the thin veneer of Recent, Pleistocene, and possibly late Tertiary sediments.

It is geologically unsound to assume that the countless bays were formed as spring vents from the *deep* Cretaceous circulation. The bays are entirely foreign to any ground-water features known elsewhere in the Cretaceous rocks of the Gulf and Atlantic coastal plains. Then, too, the bays give no sign of being related to the fracture-and-fault pattern that is always found in rocks of any appreciable geologic age. Such a genetic relationship of sinkholes and solution basins to the joint-and-fault pattern of bedrock, even of gently dipping strata truncated by erosion, is a common feature in karst regions the world over. Downward-moving ground water usually searches out rock weaknesses, such as fractures and faults. If there are upward-moving currents of ground water or spring vents anywhere in the world large enough to produce features such as karst topography or such as the still larger elliptical and oval basins under discussion, they should also be closely related to the joint-and-fault pattern of the bedrock. However, even small features, formed by rising ground water, are rare; and large features formed in the same way and comparable to the basins under discussion are not known to exist anywhere in the world.

Furthermore, the Cretaceous rocks, where they are not actually exposed, are truncated by one or more unconformities at fairly shallow depths, usually measurable in hundreds of feet, so that if the shallow deposits were removed, the position of the bays would be distributed across different Cretaceous formations. Yet they show no regular relationship to the distribution of these formations. The rate of truncation of the Cretaceous is undoubtedly variable but is thus far unknown. It probably will vary from 10 to 40 feet per mile, judging by relationships elsewhere in the Gulf coastal plain (Applin and Applin, 1947). Ten feet per mile is a fairly rapid truncation—in 10 miles 100 feet of strata would crop out on a level surface. According to all that is known about the activity of ground water, either ascending or descending, the sinkholes and spring vents formed by solution should, to a marked degree, be related to these outcrops and to the rock fractures which cut through them. That the bays are not so related shows that the hypothesis dealing with spring vents from Cretaceous artesian water is not applicable to the origin of the bays.

Johnson failed to consider the stratigraphy of the Cretaceous rocks of the coastal plain and also the facies or lithology changes which might greatly retard the movement of ground water along the strata. Oil geologists working with the subsurface stratigraphy of the Atlantic and eastern Gulf coastal plains report that such facies changes are important.[4]

[4] Mrs. Esther R. Applin, personal communication.

The *shallow* artesian circulation cannot be regionally systematic, as the shape and distribution of the bays would require, but is rather confined to local basins of irregular size and shape. The existence of a group of local and relatively shallow artesian basins is indicated by (1) the complicated, though small-scale, topography of the Pleistocene shore deposits and by the extremely irregular strata that must exist in them and (2) by the irregular distribution of the few Tertiary formations found in

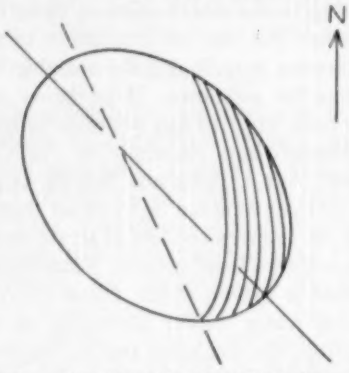

FIG. 1.—The effect of beaches built in the eastern part of lakes by the prevailing southwesterly winds has changed the shape and apparent elongation of many bays.

that part of the coastal plain in North and South Carolina where the bays are best displayed. It is accordingly unsound to assume that basins of the size, uniform shape, and regional extent of the elliptical and oval bays were formed by upward-moving ground water of the shallow circulation. It is even more unsound to assume that the bays were formed by upward-moving Cretaceous and Eocene ground water, in view of facts presented above and in the following section.

II. Johnson assumed implicitly that the coastal-plain bedrock (chiefly Cretaceous) dips down the slope of the coastal plain toward the southeast, much as one finds in textbook illustrations of the ideal coastal plain. The true direction of dip of these bedrock formations, however, is toward the south or slightly southwestward (Cooke, 1936, geologic map; Applin and Applin, 1947) and not toward the southeast, as Johnson assumed. Therefore, if spring vents moved up-dip, as Johnson assumed, they should have moved northward, producing a north-south elongation of the assumed vents instead of a northwestward elongation as the shape of the bays requires.

III. Johnson's requirements of frequent slumping and soil flow into the shallow vents far outdistances the real capabilities of the surficial Pleistocene and Recent deposits for such activity, not to mention the still smaller tendency toward surface flowage of the more tightly packed bedrock of Tertiary and Cretaceous age.

IV. Johnson stated (1942, p. 185) that a very few of the oval bays trend slightly west of south instead of southeastward. The effect of beaches built in the lakes under the influence of prevailing southwesterly winds has changed the shape of many bays, as shown by figure 1 and as is clearly seen in many of the aerial photographs. Then, too, paired elliptical bays are quite common (fig. 2).

Where the details are partly obscured, one might interpret an axis with northeasterly trend instead of two bays with northwesterly trend. This is no doubt what Johnson did, inasmuch as the writer has never been able to find an elliptical bay with northeasterly trend in the Carolinas.[5]

V. Considering the volume of finer

[5] There may be certain small sinkholes of many different elongations, but these can usually be separated from the larger bays.

sediments which would have to be removed from the bays according to the spring-sapping phase of Johnson's theory, it is evident that many of the smaller upland bays should be partially or wholly filled with stream deposits and with small deltas of material washed out of the large bays. Such is not the case. Very little muddy water has issued from lake outlets in the past, and practically none is visible today. There should also be a well-developed drainage system on the uplands, but this, too, is usually not present, as McCampbell (1944) has pointed out.

VI. Johnson speaks repeatedly of westerly winds forming dunes on lake beaches, implying that the effective sand-moving winds came partly from the northwest as well as from the southwest. A careful study of the dune forms of the Carolina coastal plain compels the conclusion that the great preponderance of sand-moving wind came from the southwest and hardly at all from the northwest. In other words, the elongation of the bays cannot be attributed to wind-scour by northwest winds without doing violence to all the dune alignments visible on the coastal plain.

VII. A classic example of Johnson's artesian spring-sapping hypothesis should be found in Texas and Louisiana, where the porous Lissie sands and gravels dip beneath the less pervious Beaumont clays and silts. It is an ideal location for spring-sapping under conditions similar to those postulated by Johnson, but not actually existing on the Carolina coastal plain. Of course, no bays of this kind are known in Texas and Louisiana.[6]

[6] There are a few shallow oval basins in and near Bee County, southern Texas. These have been described as "clamshell" in shape. They have irregularities of shape which seem definitely to relate them to the prevailing winds, as is also the case with many large oval basins in the High Plains of Texas.

ADDITIONAL FACTS ABOUT THE "BAYS"

Following are some of the facts which could have been ascertained by a careful study of the aerial photographs or by field work:

1. The bays "fade out" away from the Cretaceous areas of the Carolina coastal plain. The disappearance is gradual, so far as the writer has observed. Basins of irregular shape continue well into Georgia, but they may be of quite different origin.

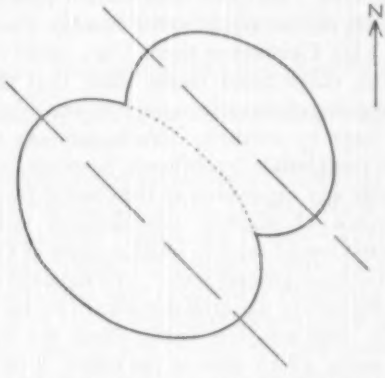

FIG. 2.—Paired bays, partly obscured, might have the appearance of one bay with northeast-southwest alignment.

2. In general, the shape of the bays is more irregular toward the southeast, i.e., toward the ocean. This is in harmony with the view that shore processes, lagoon processes, and other associated changes did not originate the bays but only changed their shape and disturbed their symmetry.

3. Certain crescent-shaped, sandy rims protrude above lagoon deposits of elevated shores, particularly of the Pamlico shore, even though the bay depressions are filled, buried, and invisible.

4. In Brunswick County, North Carolina, beach ridges of an elevated marine

shore, probably the Pamlico,[7] have apparently been constructed across many large and small bays. Later subsidence of the beach-ridge material into voids, probably caused by descending ground water, has caused these beach ridges to settle slightly into the bays, yet to remain visible. Either physical rearrangement or solution of the bay-filling, or both together, could have been the cause of the subsidence of the ridges. The ridges are still visible in many cases but are almost concealed by the swamps within the bays. This dates these buried bays at an age at least as old as the Pamlico shore of Late Pleistocene time. They could be much older. Some might think that the foregoing observations point to the origin of bays by solution. This is only one of the permissible hypotheses, however, because any depression in the coastal-plain surface of slightly consolidated sediments would tend to localize some of the activity of ground water. The removal of sedimentary material downward by solution does not necessarily cause the depression which collects the water; it may be the result of a depression which existed there at the beginning of the process.

Johnson presented an analysis of minor geomorphic processes which, without doubt, modified the bays but which are inadequate to originate them. For example, he shows that some of the accessory bay features, such as multiple rims of sand, are probably old lake beaches or combinations of lake beaches and sand dunes derived from them. This fact was not apparent in 1933, when the only aerial photos available were of a small district in Horry County, South Carolina. But by 1938 the truth about many of the rims became apparent to those who saw the U.S. Department of Agriculture aerial photographs, which by that time covered nearly the entire coastal plain. Much of what Johnson says about the sandy rims of the bays and of other surficial features is a worthwhile contribution to Pleistocene and Recent geologic history. But these observations have no necessary relationship to a revised meteoritic theory, either pro or con.

[7] Published data do not permit an accurate determination of which of the two lowest terraces is involved at this place.

NEW PHOTOGRAPHS SHOWING THE BAYS

The following mosaics of aerial photographs ("photo-index-sheets") illustrate the bays in the region where they are well displayed. It is suggested that persons interested in this problem purchase these in order to see for themselves the unusual conditions that demand explanation.[8]

North Carolina	Sheet No.
Bladen County	2
Brunswick	6
Cumberland	4
Hoke	1
Robeson	2 (north)

Aerial photographs of the northern Alaska coastal plain, made recently by the Army Air Forces and the U.S. Navy, show many oval, circular, and even rectangular lake basins of irregular outline. These have been explained by Cabot (1947) as the result of slumping into voids created by the melting of the thick ground-ice and of related changes in the soil. The Carolina bays might conceivably be explained in a similar way, were it not true that the necessary refrigeration of climate would be out of harmony

[8] These sheets are available on double-weight photographic paper for $1.00 each from: Aerial Photographic Laboratory, Aerial Photographic and Engineering Service, Production and Marketing Administration, U.S. Department of Agriculture, Washington 25, D.C.

with the known and inferred past climates of the Carolina coastal plain.

JOHNSON'S CONTRIBUTION TO THE LOGIC OF GEOLOGY

Johnson gave a number of addresses in eastern and midwestern universities, using his theory of the Carolina bays as an example of what can be done by the energetic use of inductive and deductive thinking and by rigorous thinking in general. His contribution to the logic of geological thinking was given unusual emphasis by the Society of Sigma Xi in the two publications of the same paper referred to above. The value of his contribution to geological logic may be judged from the following brief observations about certain critical points:

a) He did not succeed in substituting a better theory in place of the meteoritic theory, since his own proposal violates the laws of the conservation of energy.

b) He did not disprove a "meteoritic" theory of origin, though he did point out the improbability of some of the minor features of the original meteoritic theory.

c) He did not revise the meteoritic theory or make any genuine attempt to do so, in spite of the fact that it obviously was in need of revision.

d) Though Johnson claimed to use the "method of multiple hypotheses," he still did not use this method in its full strength, since he persistently neglected to revise the meteoritic theory.

e) Since he was primarily interested in the surface form of geological features, Johnson underestimated the significance of the subsurface aspects of geology for this problem.

THOUGHTS ON REVISION OF THE METEORITIC HYPOTHESIS

The bays could have been formed by meteoritic impact at several times in the geologic past, as far back as Cretaceous time, whereas the bay rims and other such features discussed by Johnson are probably largely of Recent origin. The following geologic times were intervals of erosion on the coastal plain when the supposed meteoritic impact could have occurred (Cooke, 1936) (there may have been others):

Late Cretaceous and Early Eocene time
All of Oligocene time except the latest
Early Miocene time (pre-Hawthorne Formation)
Middle Miocene time (post-Hawthorne Formation)
Early Pliocene time (pre-Waccamaw Formation)
Latest Pliocene time (post-Waccamaw Formation)
In two or three of the early and middle Pleistocene emergences

Judging from the published knowledge of the distribution of the Tertiary and early Pleistocene formations of the Carolina coastal plain, the assumed meteoritic impact could have occurred not only at any one of the above intervals of erosion but also during any one of the intervening times of subsidence and sedimentation. It is unlikely that the event could have occurred after Middle Pleistocene time, since beach ridges of one of the younger sea invasions were apparently deposited across the elliptical bays in Brunswick County, North Carolina, and elsewhere. If the bays had been formed at any of these times of coastal-plain emergence before the Pleistocene, they could have been subsequently filled with sediments during times of submergence. During times of emergence the bays would, no doubt, have been evident on the surface, though they might have been very obscure, as they are today in certain places. Ground water moving either upward or downward, though predominantly downward, would be expected to rearrange the

sediments partially by solution and partially by physical movement of the finer particles. A very slight subsidence of bay-filling sediments would have an effect upon the surface, particularly upon a surface as smooth as that of the uplands in the coastal plain. The size of many bays is so great in comparison to the thickness of Tertiary or Quaternary sediments that it is unthinkable that they should remain completely concealed for long. That slight movements have noticeable surface expression on very flat surfaces is demonstrated by the surface trace of faults and other structural features in the western Gulf Coastal Plain of Texas and Louisiana.

ABOUT UNIFORMITARIANISM

It is apparent that several writers, including Johnson, feel that a theory involving a cometic or meteoritic impact would violate the "principle of uniformitarianism." Most astronomers do not consider such an event improbable; perhaps the vastness of the available geologic time has influenced them in this decision.

The doctrine of uniformitarianism has, without doubt, been the main logical principle in geologic thinking throughout much of the development of the science. It is not clear nowadays, however, just what is the position of uniformitarianism in all geologic questions. In many it needs clarification before it has meaning. Its greatest use is perhaps as a guide in the selection of fields of thought at the time when the first theories of origin are being formulated. For complete geological studies, scientific method should be followed, irrespective of uniformitarianism. After all, the aim of geological thinking is not the formulation of theories but the unraveling of a true account of geologic history.

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SYNTHETIC SAPPHIRINE AND ITS STABILITY RELATIONS IN THE SYSTEM MgO-Al_2O_3-SiO_2[1]

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ABSTRACT

A compound corresponding in optical properties and in X-ray powder pattern with the natural mineral sapphirine has been synthesized by repeated sintering of the constituent oxides at atmospheric pressure. The most probable formula for this artificial compound is $4\ MgO \cdot 5\ Al_2O_3 \cdot 2\ SiO_2$. An unidentified crystalline phase previously noted in certain magnesia-alumina-silica porcelains has been shown to be identical with this synthetic sapphirine. There is reason to believe that sapphirine possesses a small field of primary crystallization on the liquidus surface of the ternary system MgO-Al_2O_3-SiO_2. Some revision of the earlier work on this system therefore appears to be necessary. Experiments designed to prove the existence of sapphirine as an artificial mineral and to determine its thermal behavior and its stability relations with other crystalline phases in the ternary system have been carried out. The results indicate that synthetic sapphirine melts incongruently at or near 1475° C. to yield spinel and liquid. Immediately below that temperature, sapphirine is compatible with cordierite, spinel, and mullite, and at still lower temperatures with corundum also. On the basis of these results a revision of the equilibrium diagram for the system MgO-Al_2O_3-SiO_2 has been made. It is hoped that this revision will considerably enhance the usefulness of the diagram to petrologists and ceramic technologists.

INTRODUCTION

In 1918, G. A. Rankin and H. E. Merwin presented a phase equilibrium diagram for the ternary system MgO-Al_2O_3-SiO_2. The data upon which it was based were obtained largely by the well-known quenching method, with petrographic identification of the crystalline phases. The only ternary compound detected as a primary phase in the system was the mineral cordierite ($2\ MgO \cdot 2\ Al_2O_3 \cdot 5\ SiO_2$). During the ensuing thirty years no major revisions of the diagram have been found necessary except those indicated by the reinvestigation of the system Al_2O_3-SiO_2 in 1924 by N. L. Bowen and J. W. Greig and by the delineation of the two-liquid area in 1927 by Greig. The diagram resulting from these revisions (fig. 1) is now generally accepted without question as the correct configuration for the system. More recently, in 1943, A. Bystrom has made an X-ray diffraction study of numerous sintered mixtures in the ternary system. He, too, found no evidence for a ternary compound other than cordierite. Numerous other workers, engaged in studies of ceramic products of the cordierite, spinel, mullite, and corundum types, have likewise failed to detect any additional ternary compound.

The natural mineral sapphirine possesses a composition falling essentially within the system MgO-Al_2O_3-SiO_2. Although characterized by partial substitution of iron for magnesium and by magnesium and silicon for aluminum, it is usually assigned such simplified formulas as $5\ MgO \cdot 6\ Al_2O_3 \cdot 2\ SiO_2$ or $2\ MgO \cdot 2\ Al_2O_3 \cdot SiO_2$. With the one or the other of these two formulas the composition of iron-free sapphirine falls either within the cordierite-spinel-mullite triangle at the point *Z* or on the cordierite-spinel join at the point *X* (fig. 2). In either case the temperature of final consolidation of the molten mineral should, on the basis of figure 1, be 1460° C., the temperature of the invariant point *3*, at which the pri-

[1] Contribution from the Research Laboratories of the Ceramic Division of the Champion Spark Plug Company. A condensation of an article submitted for simultaneous publication in the *Journal of the American Ceramic Society*. Manuscript received October 17, 1949.

mary fields of cordierite, spinel, and mullite come together. But this temperature is considerably below the recorded melting temperature of natural sapphirine. F. C. Kracek (1942) reported that sapphirine from Val Codera, Italy, melted incongruently at 1580° C. to give spinel and liquid. C. Palache, H. Berman, and C. Frondel (1944, pp. 724–726) stated that sapphirine (source unspecified) melted incongruently at 1500° C., yielding spinel, sillimanite,[2] and a siliceous glass. Why, then, should not sapphirine occur as a primary phase in the system MgO-Al_2O_3-SiO_2? One possible explanation is that sapphirine might be one of the so-called "pressure-minerals" and therefore would not form at atmospheric pressure. Another is that the pure magnesian end-member might not exist as a distinct mineral entity, an improbable relation in view of the existence of natural sapphirine that is very low in iron. Unless the one or the other of these assumptions is valid, however, the conclusion appears to be inescapable not only that the synthesis of sapphirine at nor-

[2] Undoubtedly this was mullite.

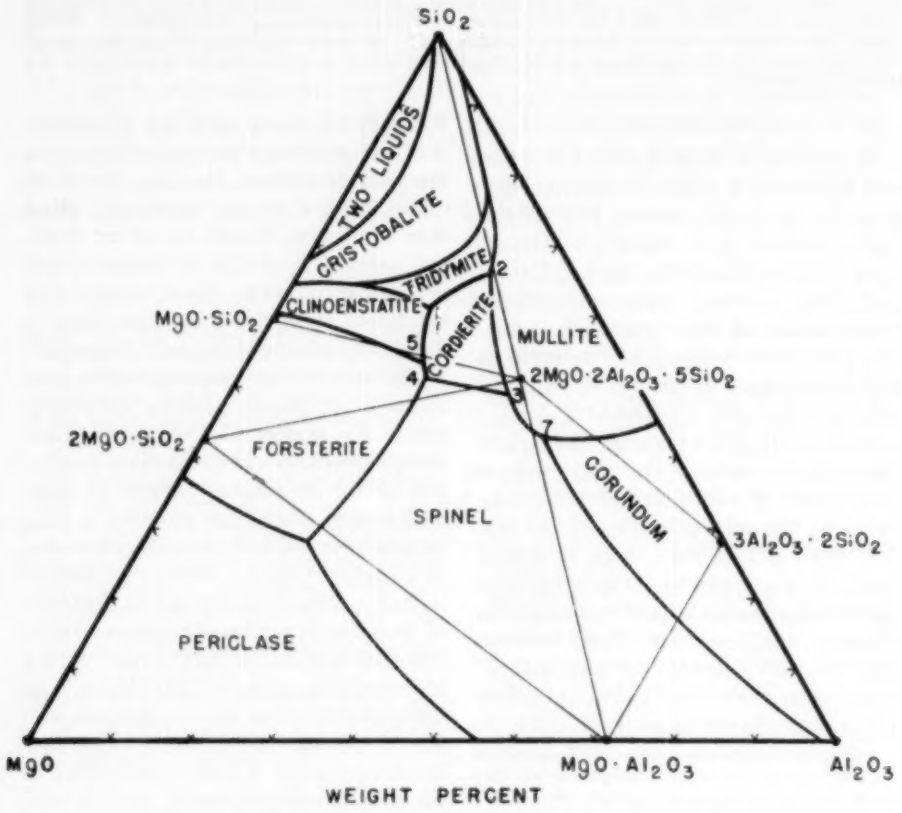


FIG. 1.—Equilibrium diagram of the system MgO-Al_2O_3-SiO_2 (after Rankin and Merwin [1918]), with revisions based on the work of Bowen and Greig [1924], and of Greig [1927]).

mal pressures should be possible but also that sapphirine should possess a field of primary crystallization in the system MgO-Al_2O_3-SiO_2.

The writer became interested in this problem during a study of certain magnesia-alumina-silica porcelains. In some of these a weakly birefringent phase with refractive indices close to that of spinel had been noted. An earlier report by A. B. Peck (1941) on similar porcelains was also found to refer to such a phase, believed by Peck to represent a spinel-mullite solid solution. The facts presented in the preceding paragraph, however, strongly suggested to the writer that this phase might be identical with the mineral sapphirine. Accordingly, it was decided to attempt to synthesize sapphirine by sintering the oxides. The failure of E. Dittler (1928) to obtain sapphirine from fusion experiments did not appear to be too ill an omen, in view of the incongruent melting behavior of natural sapphirine. When the sintering experiments proved successful, it was decided also to determine the chemical formula and thermal behavior of synthetic sapphirine and to ascertain its stability relationships with other phases in the system MgO-Al_2O_3-SiO_2.

SCOPE AND METHOD OF INVESTIGATION

For present purposes it was considered necessary to reinvestigate only that portion of the system MgO-Al_2O_3-SiO_2 covered by the quadrilateral cordierite-spinel-corundum-mullite. The composition of natural sapphirine, although somewhat variable, strongly indicated that its synthetic counterpart would almost certainly fall within this area. Furthermore, the occurrence of sapphirine in this portion of the system would threaten the validity of only those compatibility relations depicted by the presently accepted equilibrium diagram (fig. 1) as existing between cordierite, spinel, corundum, and mullite. There appeared to be no reason to question the stability relations indicated for compositions falling outside this quadrilateral.

Approximately sixty compositions in the cordierite-spinel-corundum-mullite quadrilateral were prepared for this study (fig. 2). As can be seen from the linear arrangement of the composition points, the majority of these mixtures fall into six well-defined series traversing the quadrilateral. This systematic choice of mixtures greatly facilitated the determination of the chemical formula of sapphirine and the subdivision of the quadrilateral into the appropriate stability triangles.

The method of investigation employed in this study differed considerably from that used by Rankin and Merwin (1918). Facilities for quenching experiments were not available to the writer. However, for the purpose at hand it is believed that the method herein employed was more successful than the quenching technique. The present method consisted of repeated sintering of appropriate oxide mixtures at selected elevated temperatures in the vicinity of 1450° C. until reaction had been completed and equilibrium established. These reactions were essentially solid-state reactions because, with few exceptions, they took place in the absence of any appreciable amount of liquid phase. In general, only X-ray diffraction methods were capable of distinguishing the extremely fine-grained crystalline phases present in the fired mixtures.

EXPERIMENTAL PROCEDURE

Magnesia, alumina, and silica of high purity were used as raw materials. All three oxides were sufficiently fine grained

to pass a 325-mesh screen. Test pieces of the various mixtures were prepared in the form of small pressed disks with an organic binder. The majority of the specimens were fired in a Dressler muffle-type tunnel kiln operating on a 3-day schedule at 1450° C. A few pieces were fired either in a tunnel kiln operating on a 35-hour schedule at 1470° C. or in a tunnel kiln operating on a 6-hour schedule at 1650° C. Thermal stability tests on synthetic sapphirine were made in a laboratory load-test gas furnace in the temperature range 1400°–1525° C. In addition, a series of precision runs were kindly made in a quenching furnace by J. F. Schairer, of the Geophysical Laboratory.

The crystalline phases present in the calcined specimens were identified almost solely from X-ray diffraction patterns. The latter were obtained with a Norelco Geiger counter recording X-ray spectrometer, using copper-target radiation. Five crystalline phases, corresponding to the natural minerals cordierite, spinel, sapphirine, mullite, and corundum, were thus recognized. Patterns for these five synthetic minerals are shown in figure 3. The polarizing microscope was of limited usefulness in the determination of phases

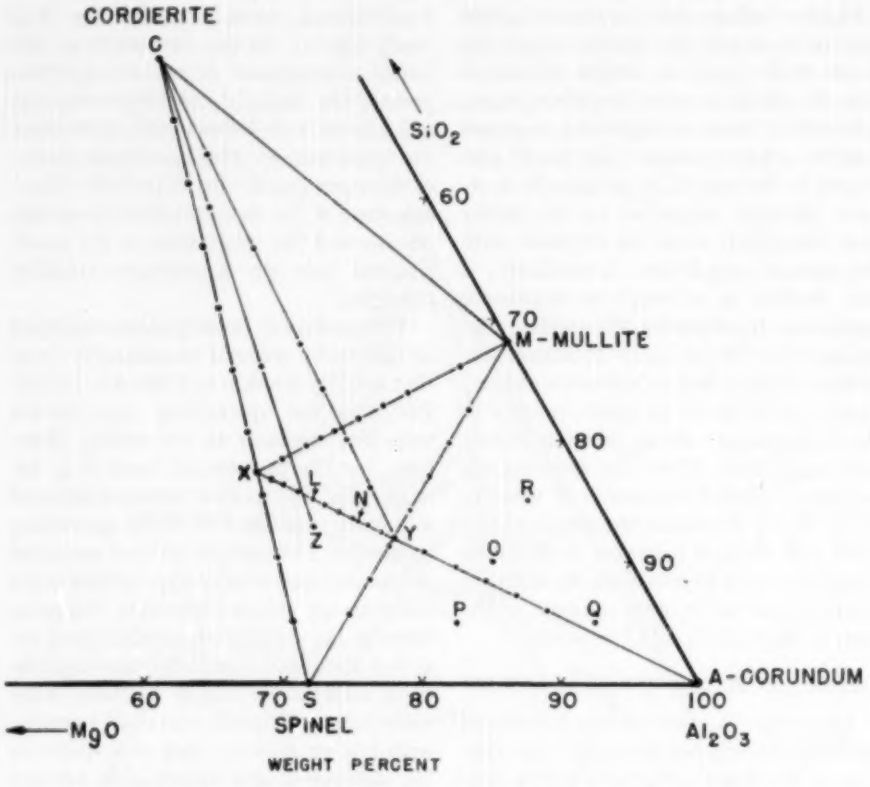


FIG. 2.—High-alumina portion of MgO-Al_2O_3-SiO_2 system, showing compositions studied

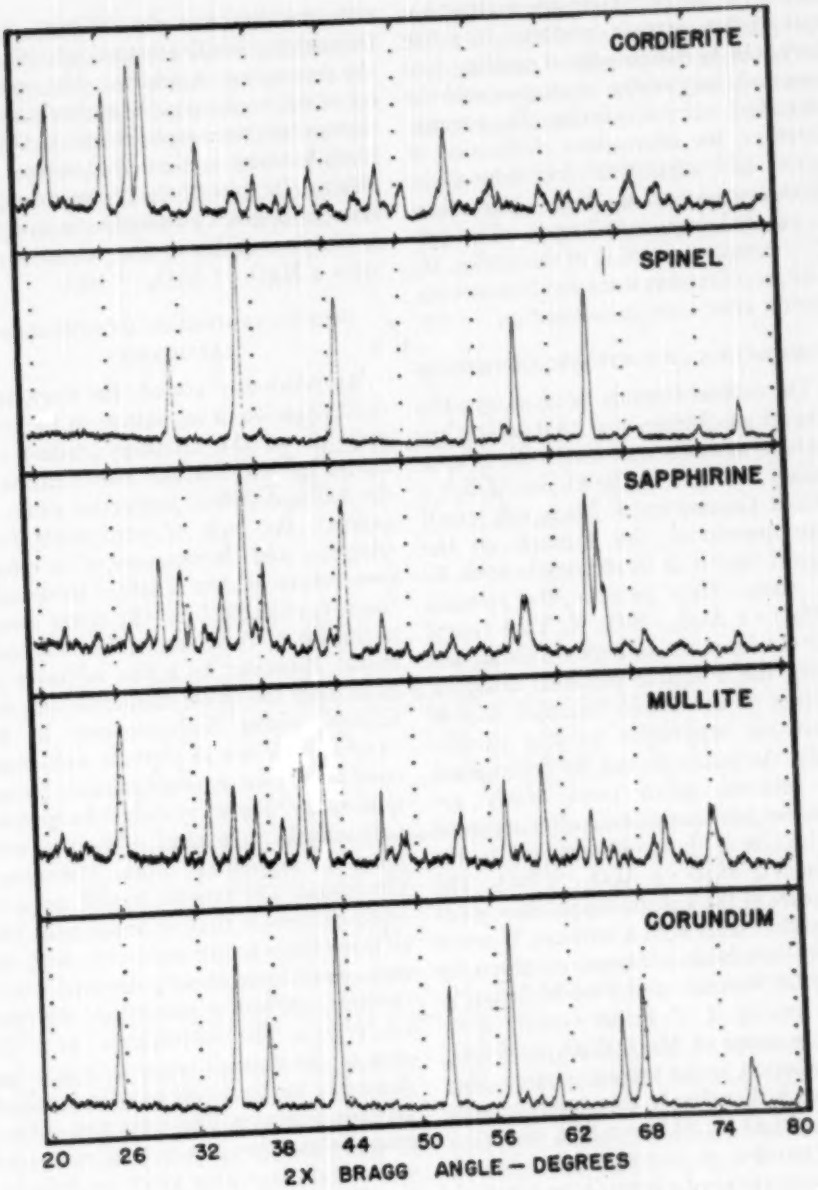


FIG. 3.—X-ray spectrometer patterns of compounds encountered

because of the extremely fine-grained nature of the reaction products. In a few instances in which partial melting had occurred, sapphirine crystals could be identified microscopically. In general, however, the microscopic distinction of spinel and sapphirine was impossible. With few exceptions, three calcinations at 1450° C. were sufficient to effect sensibly complete reaction of the oxides. Indeed, reaction was remarkably near completion after a single calcination.

COMPOSITION OF SYNTHETIC SAPPHIRINE

The earliest formula to be assigned to natural sapphirine, and that which appears in most current textbooks of mineralogy, is $5\ (Mg,Fe)O \cdot 6\ Al_2O_3 \cdot 2\ SiO_2$. B. Gossner and F. Mussgnug (1929) have questioned this formula on the ground that it is incompatible with X-ray data. They propose the formula $2\ MgO \cdot 2\ Al_2O_3 \cdot SiO_2$. T. Vogt (1947) also considers the latter formula as best fitting the available chemical analyses. Neither of the above formulas is indicated for sapphirine by the present study. As nearly as can be determined, the mixture which most nearly approaches pure sapphirine after repeated calcination is that corresponding to the formula $4\ MgO \cdot 5\ Al_2O_3 \cdot 2\ SiO_2$. The evidence of the natural sapphirines is not in conflict with such a formula. If seven of the available analyses, as given by Palache, Berman, and Frondel (1944, p. 725) and by R. T. Prider (1945), be recast in terms of MgO, Al_2O_3, and SiO_2, and plotted in the ternary system, they cluster rather closely about the composition $4\ MgO \cdot 5\ Al_2O_3 \cdot 2\ SiO_2$ (fig. 4).

A number of factors render difficult the assignment of a formula for synthetic sapphirine with absolute certainty. These include the lack of absolute purity of the reagents, the failure to achieve 100 per cent reaction, and the difficulty of distinguishing small amounts of spinel in the presence of sapphirine. The possibility of solid-solution effects should not be discounted, since replacement of AlAl by MgSi has been attributed to natural sapphirine. In spite of the above considerations, it appears reasonable to assign to synthetic sapphirine the tentative formula $4\ MgO \cdot 5\ Al_2O_3 \cdot 2\ SiO_2$.

OPTICAL PROPERTIES OF SYNTHETIC SAPPHIRINE

As previously stated, the fineness of crystallization of sapphirine in the great majority of the mixtures studied precluded its detection and identification on the basis of optical properties alone. In general, the lack of perceptible birefringence and the nearness of its refractive indices to that of spinel rendered it indistinguishable from the latter except by means of the X-ray diffraction technique. However, in a few mixtures located near the spinel-cordierite join, sufficient melting had occurred in the 1450° C. calcines to produce well-developed sapphirine crystals suitable for optical study. These crystals corresponded very closely in optical properties with low-iron sapphirine from Fiskernaes, Greenland. The typical crystal shape in these calcines is that of broad laths two or three times longer than wide, with the ends sometimes bluntly pointed. Such crystals reached a maximum observed size of 75 μ. The birefringence, as in the case of the natural mineral, is low but distinctly visible in all but the smallest crystals or grains. The refractive indices range from 1.705 to 1.712, as compared with 1.7055 to 1.7112 given by Palache, Berman, and Frondel (1944, p. 724) for the almost iron-free Greenland variety. No suitable interference figures were obtained. Extinction appears to be parallel

or essentially so in most of the laths, but inclined extinction up to an estimated maximum of 10° or 12° can also be observed. Laths showing inclined extinction apparently exhibit only positive elongation. So also do most of those showing parallel extinction, although negative elongation is displayed by a small proportion of the laths. Synthetic sapphirine, was the recognition, in certain porcelains, of crystals possessing optical properties similar to those given above which led to the present investigation of synthetic sapphirine.

X-RAY DIFFRACTION DATA

A glance at figure 3 clearly reveals that the pattern assigned to synthetic

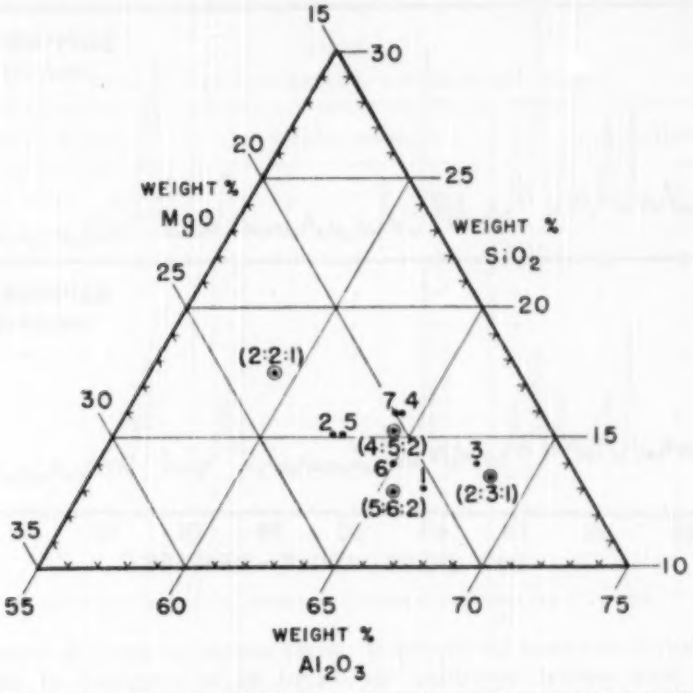


FIG. 4.—Compositions of seven natural sapphirines compared to several possible formulas. (1) Fiskernaes, Greenland, (2) Itrongay, Madagascar, (3) Transvaal, South Africa, (4) Val Codera, Italy, (5) Vizagapatam, India, (6) St. Urbain, Quebec, and (7) Dangin, Western Australia.

as might be expected from its purity, lacks the characteristic color and pleochroism of the natural mineral. Otherwise its properties, as recorded above, leave little doubt as to its identity and serve as confirmation, if such be needed, of the X-ray diffraction evidence for the synthetic development of sapphirine in these calcines. It will be recalled that it sapphirine is distinctly different from those of each of the other four crystalline phases. There are, nevertheless, strong points of similarity between the patterns of sapphirine and of spinel. In order to prove that the sapphirine pattern was not due to a mechanical mixture of spinel, cordierite, and mullite, it was compared to that of a mixture of these

three minerals in the proportions equivalent to sapphirine. The two patterns could not be correlated. As an added precaution, the sapphirine pattern was compared to those of several alumina-rich spinel solid solutions. Here again the patterns did not correspond, thus disposing of any belief that sapphirine might be merely such a spinel solid solution.

as to the development of artificial sapphirine in the calcines prepared for this investigation. The presence of sapphirine in certain magnesia-alumina-silica porcelains was also indicated by X-ray diffraction analysis.

Because of the similar locations of the main peaks of spinel and sapphirine, it is difficult to disprove the presence of a

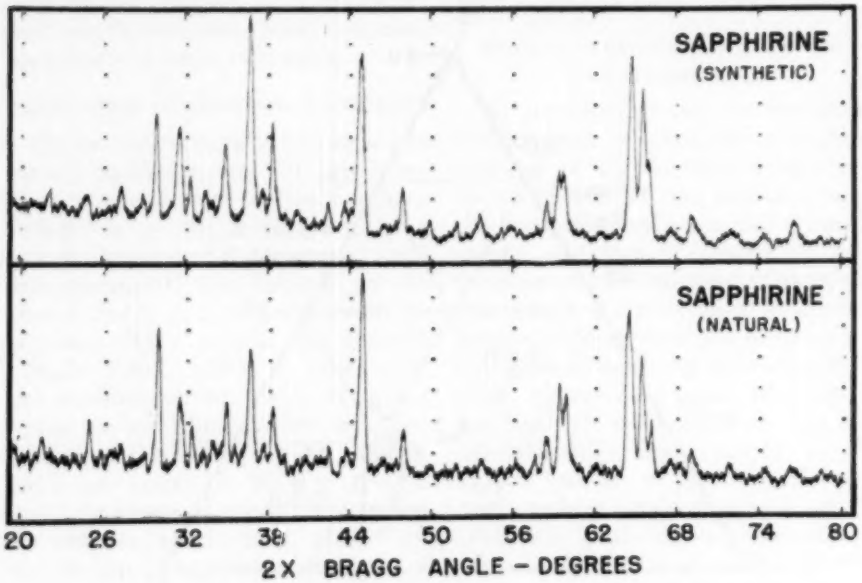


FIG. 5.—X-ray spectrometer patterns of synthetic and natural sapphirine

In order to demonstrate the identity of synthetic with natural sapphirine, its pattern was compared with that of sapphirine from Fiskernaes, Greenland. Careful scrutiny of the two patterns (fig. 5) indicates that they are practically identical in every detail. In table 1 are presented the interplanar spacings and relative intensity data for natural and synthetic sapphirine, as well as for synthetic spinel. The data presented in table 1 and the patterns from which they were derived (figs. 3 and 5) leave no question

small amount of spinel in a calcine believed to be composed of sapphirine alone. The assignment of an accurate formula for sapphirine is therefore very difficult. So also is the accurate determination of the temperature at which sapphirine begins to melt to yield spinel and liquid. In this connection, the most distinctive region of composite patterns of the two minerals is between 64° and 66° ($2 \times \theta$). As spinel increases at the expense of sapphirine, the 1.424 peak of spinel crowds up between the twin peaks

1.435 and 1.419 of sapphirine. The spinel peak appears to merge with the 1.419 peak, gradually increasing its intensity beyond that of the 1.435 peak of sapphirine. Just prior to complete disappearance of sapphirine, the 1.435 and 1.410 peaks of the latter have been reduced to very minor peaks, one at either side of the now intense 1.424 spinel peak.

THERMAL STABILITY

The thermal stability of synthetic sapphirine is of considerable significance in regard to the possible need for revision of the equilibrium diagram of the system MgO-Al_2O_3-SiO_2. The entire question as to whether sapphirine should possess a field of primary crystallization on the liquidus surface of the system hinges di-

TABLE 1

X-RAY DIFFRACTION DATA FOR SAPPHIRINE AND SPINEL

Sapphirine (natural)		Sapphirine (synthetic)		Spinel (synthetic)	
d	Relative Intensity	*d*	Relative Intensity	*d*	Relative Intensity
				4.60	0.3
4.10	0.1	4.01	0.1		
3.56	0.2	3.58	0.1		
3.26	0.1	3.25	0.1		
3.07	0.1	3.07	0.1		
2.97	0.7	2.98	0.5		
2.83	0.4	2.82	0.4	2.83	0.4
2.75	0.2	2.76	0.2		
2.68	0.1	2.67	0.2		
2.63	0.1	2.62	0.1		
2.56	0.3	2.56	0.4		
2.52	0.1				
2.44	0.6	2.44	1.0	2.42	1.0
2.38	0.1	2.38	0.2		
2.34	0.3	2.34	0.5	2.32	0.1
2.21	0.1	2.23	0.1		
2.12	0.2	2.12	0.1		
2.06	0.2	2.06	0.1		
2.01	1.0	2.01	0.9	2.01	0.8
1.95	0.1	1.94	0.1		
1.89	0.2	1.89	0.2		
1.82	0.1	1.82	0.1		
1.757	0.1	1.754	0.1		
1.702	0.1	1.703	0.1		
1.632	0.1	1.638	0.1	1.643	0.2
1.594	0.1				
1.574	0.2	1.571	0.2	1.574	0.1
1.549	0.5	1.549	0.3	1.550	0.7
1.538	0.4	1.542	0.3		
1.488	0.1	1.480	0.1		
1.438	0.9	1.435	0.9		
1.420	0.6	1.419	0.7	1.424	0.8
1.408	0.3	1.410	0.4		
1.382	0.1	1.381	0.1		
				1.357	0.1
1.355	0.2	1.354	0.1	1.354	0.1
1.310	0.1	1.307	0.1		
1.270	0.1	1.270	0.1	1.274	0.1
1.242	0.1	1.239	0.1	1.230	0.2

rectly on its temperature of melting. If it can be demonstrated that synthetic sapphirine is stable for even just a few degrees above the temperature (1460° C.) of point 3 (fig. 1), at which a melt of any composition within the cordierite-spinel-mullite triangle should complete its crystallization, the existence of a primary sapphirine field can be considered as established.

An attempt was made to establish approximately the temperature of melting of synthetic sapphirine. Specimens of the 4 $MgO \cdot 5\ Al_2O_3 \cdot 2\ SiO_2$ mixture, calcined at 1450° C., and giving a sensibly pure sapphirine pattern, were reheated at 10° intervals in the temperature range 1450°–1500° C. in a laboratory furnace. In runs of 30 minutes' duration the lowest temperature at which spinel formation could be detected was 1480° C. Confirmatory runs were kindly made by J. F. Schairer, of the Geophysical Laboratory, in one of his precision quenching furnaces. Table 2 summarizes the results of X-ray spectrometer analyses of six runs made by Schairer on samples of synthetic sapphirine submitted to him by the writer.

TABLE 2

MELTING TEMPERATURE DATA ON SYNTHETIC SAPPHIRINE

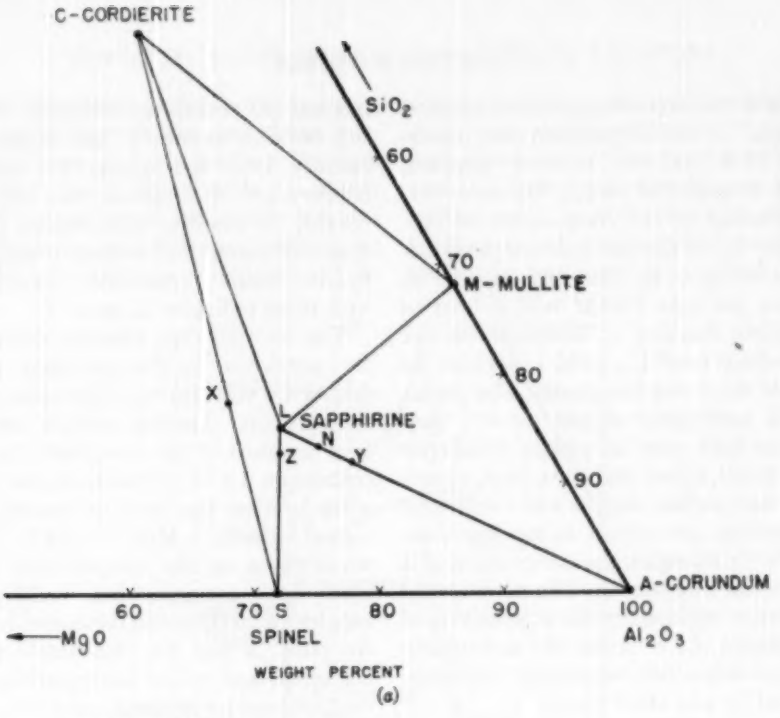
Temp. (° C.)	Time (Hours)	Crystalline Phases Present (X-ray Spectrometer)
1465....	72	Sapphirine only
1470....	7	Sapphirine with very minor spinel
1470....	42	Sapphirine with very minor spinel
1475....	31	Sapphirine with substantial spinel
1480....	7	Sapphirine with substantial spinel
1490....	7	Spinel with minor sapphirine

The lowest temperature at which the development of spinel could be definitely detected was 1470° C. Since the 42-hour run revealed no greater development of spinel than did the 7-hour run at the same temperature, the writer believes that the small amount of spinel noted in these two runs may be due to the localized fluxing action of the small amounts of impurities in the original raw materials. Spinel does not appear to develop in substantial amount below 1475° C., and the latter temperature is believed to be a reasonable choice as the incongruent melting point of synthetic sapphirine.

A puzzling feature of the thermal stability experiments of both the writer and Schairer is that mullite fails to appear along with spinel as a product of the incongruent melting of sapphirine. The location of sapphirine within the cordierite-spinel-mullite triangle would lead one to expect mullite, spinel, and cordieritic liquid to be formed. That the liquid is cordieritic is indicated by devitrifying the specimen from a 1490° C. run at 1300° C. for 1½ hours. Cordierite is thereby induced to crystallize from the glass which formed the matrix of the spinel crystals of the 1490° C. run. Mullite is entirely absent from all the thermal-stability runs, however. Strangely enough, when a 1490° C. specimen was refired at 1450° C. on a 3-day cycle through the Dressler kiln, the partial reconstitution of sapphirine from the spinel and glass was accompanied by the definite formation of mullite. The explanation of these apparently contradictory observations concerning mullite is not apparent.

COMPATIBILITY RELATIONS

In figure 6 are depicted the compatibility relationships of sapphirine with other crystalline phases in the system MgO-Al_2O_3-SiO_2, as deduced from the data accumulated in this investigation. The relationships indicated as existing below 1460° C. were obtained from a



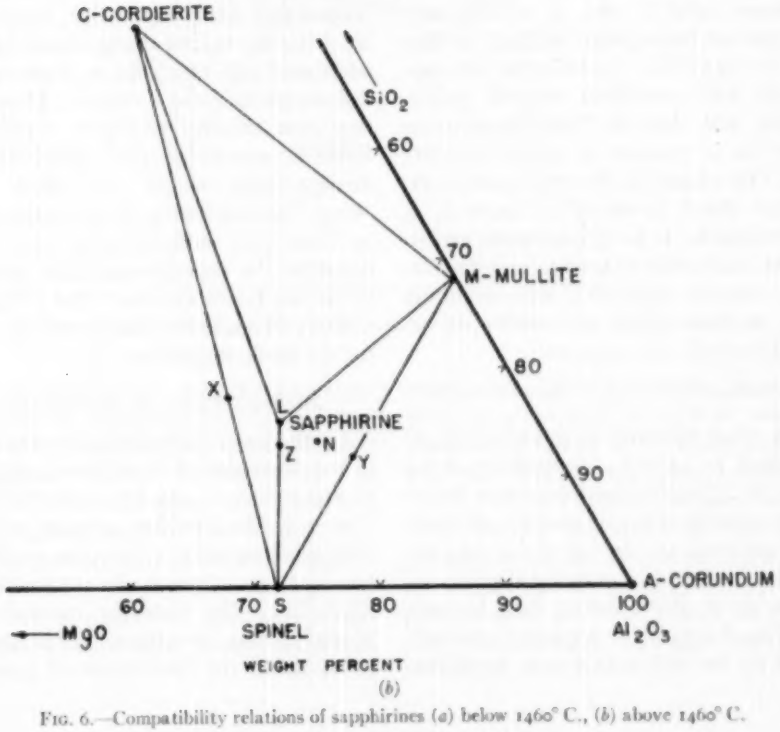


FIG. 6.—Compatibility relations of sapphirines (*a*) below 1460° C., (*b*) above 1460° C.

study of the sixty-odd specimens calcined at 1450° C. The composition quadrilateral, *CSAM*, is divided into four compatibility triangles by joining the point corresponding to the composition of sapphirine (*L*) to the four apices of the quadrilateral (fig. 6, *a*). Thus all compositions within the area *CSAM* will, if fired to complete reaction at temperatures not exceeding 1460° C., yield sapphirine as one of the crystalline phases. The phases which accompany sapphirine will vary among four pairs of phases (cordierite and spinel, spinel and corundum, corundum and mullite, mullite and cordierite), depending upon where, in the four compatibility triangles, the composition of a particular mixture lies. The more nearly a mixture approaches the composition of sapphirine (*L*), of course, the more nearly it approaches pure sapphirine, unaccompanied by any other phases.

Above 1460° C. and up to the temperature of incongruent melting of sapphirine (1475° C.), it is believed that sapphirine and corundum become incompatible and that the sapphirine-corundum join is replaced by a spinel-mullite join. This change in the relationships described above is shown in figure 6, *b*. There appears to be at least some experimental indication of such a reaction relation, whereby sapphirine and corundum react to form spinel and mullite, in accordance with the equation:

Sapphirine + corundum $\rightleftarrows$ spinel + mullite.

Thus, when mixtures *O*, *Q*, and *R* (fig. 2) are fired at 1470° C., sapphirine shows signs of instability and decreases somewhat, whereas spinel increases in amount, as compared to the 1450° C. calcines. That this effect is due to the reaction relation given above rather than to mere melting of sapphirine appears to be indicated by the fact that a pure sapphirine mixture (*L*) reveals no detectable difference between its 1450° C. and its 1470° C. calcines. Once the incongruent melting temperature of sapphirine has been exceeded, the stability relationships in the quadrilateral cordierite-spinel-corundum-mullite would presumably correspond with those indicated in figure 1.

The compatibility relations described for sapphirine in the preceding paragraphs are very directly dependent upon the particular chemical formula that has been assigned to the compound. In this connection it is of interest to inquire as to what bearing the latest previously accepted formula, $2\ MgO \cdot 2\ Al_2O_3 \cdot SiO_2$, would have on the compatibility relations. Such a composition would place sapphirine on the cordierite-spinel join at the point *X* (fig. 6). This would make cordierite and spinel incompatible, for two subliquidus systems, cordierite-sapphirine and spinel-sapphirine, would exist. A further radical change would be the additional join which must, of necessity, link sapphirine and forsterite. Thus two new compatibility triangles—cordierite-forsterite-sapphirine and spinel-forsterite-sapphirine—would be called into being. The fact that no such relationships as those just outlined have been indicated by the experimental data appears to be conclusive evidence that $2\ MgO \cdot 2\ Al_2O_3 \cdot SiO_2$ is not the correct formula for synthetic sapphirine.

PRIMARY FIELD OF SAPPHIRINE

On the basis of the evidence presented in the discussion of the thermal stability of sapphirine, it can be concluded that there must be a field of primary crystallization of sapphirine, however small, on the liquidus surface of the system MgO-Al_2O_3-SiO_2. The sintering method employed in this investigation is incapable of outlining the boundaries of primary

fields. To obtain data for this purpose, one must resort to the quenching technique. However, it is possible on the basis of the present experiments to obtain some conception of the general features of the postulated field of sapphirine. Some of the information accumulated on the cine containing 4 per cent by weight of Al_2O_3 over the sapphirine ratio yielded both spinel and mullite upon heating at 1490° C. There must, therefore, be an invariant point at which the fields of sapphirine, spinel, and mullite come together. In figure 7 this invariant point

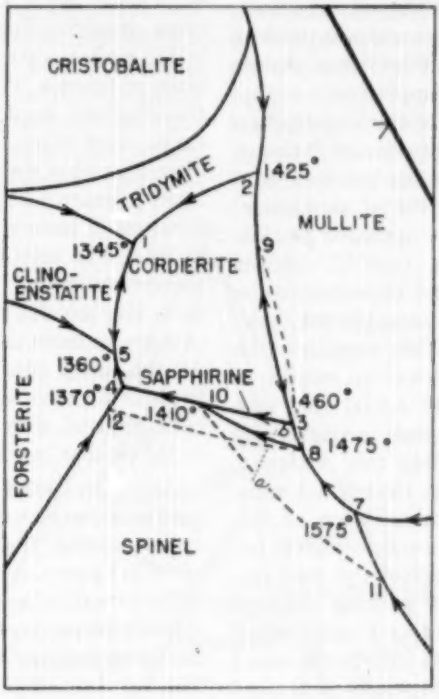


FIG. 7.—Diagram showing several possible configurations of the primary field of sapphirine

thermal relations of various calcines appears to be significant in this regard.

It will be recalled that when synthetic sapphirine melts incongruently at 1475° C., it yields spinel and liquid but no detectable mullite. Nevertheless, sapphirine, mullite, and spinel were found to coexist on subsequent refiring, at 1450° C., of a 1490° C. specimen of sapphirine. Furthermore, a sapphirine calmust fall on the spinel-mullite boundary curve at some such location as point *8*. There must also be an invariant point at which the fields of sapphirine, spinel, and cordierite come together because certain of the calcines indicated that these three phases are compatible and form a stability triangle. If the primary field of sapphirine were some such area as *8-3-9*, point *3* would be such an invariant point.

But if this were the case, the temperature of point *3*, 1460° C., would be the temperature of final consolidation of spinel-cordierite mixtures. This is at variance with the experimental facts, inasmuch as spinel-cordierite mixtures showed definite shrinkage and melting at least as low as 1410° C. It is necessary, then, to assume that the fields of sapphirine, spinel, and cordierite meet at some such point as *10*. The areas *8-3-10* would then outline the primary field of sapphirine.

During the course of the investigation two other areas, *11-7-3-10* and *8-12-4-3*, suggested themselves as possible configurations for the field of sapphirine. The first of these two appeared possible when certain of the 1450° C. calcines showed sapphirine and corundum to be compatible. Such an arrangement, however, would require that sapphirine be stable to temperatures in excess of 1575° C. (point *7*). In actual fact, sapphirine was found to melt incongruently at 1475° C. In rejecting this configuration, it was necessary, though not without some experimental evidence, to postulate that sapphirine and corundum become incompatible below the temperature of incongruent melting of sapphirine. The area *8-12-4-3* could represent the primary field only in the event that sapphirine and forsterite were compatible and spinel and cordierite were incompatible. Such relations would hold if the composition of sapphirine fell on the spinel-cordierite join, as is true of the most recently accepted formula for sapphirine, $2\ MgO \cdot 2\ Al_2O_3 \cdot SiO_2$. However, it has already been shown, in the discussion of the composition of synthetic sapphirine, that the compatibility relations were inconsistent with any such formula. So also are they inconsistent with the choice of the area *8-12-4-3* as the primary field of sapphirine.

REVISION OF MgO-Al_2O_3-SiO_2 DIAGRAM AND ITS SIGNIFICANCE

The new and hitherto unsuspected relations uncovered in this investigation indicate the need for some revision of the presently accepted equilibrium diagram for the system MgO-Al_2O_3-SiO_2 which is depicted in figure 1. Such a revised diagram, incorporating the data presented in the earlier portions of this paper, appears as figure 8. The tentative nature of this diagram should be emphasized, because more careful work may possibly alter somewhat the outlines of the postulated primary field, as well as the temperature of incongruent melting of sapphirine. The relation between the sapphirine-corundum and spinel-mullite joins also remains a challenge that may perhaps be more satisfactorily answered. At the same time it is felt that the broader features of the problem have been resolved with reasonable certainty in the present investigation.

As far as primary crystallization in the system is concerned, figure 8 calls for no radical change from the behavior indicated in figure 1. A very small area which hitherto was believed to give spinel as a primary phase must be taken away from the spinel field and allotted to sapphirine. The loss thus inflicted on the large spinel field is scarcely noticeable. However, the effect of the inclusion of sapphirine upon the end-products of crystallization is very pronounced and out of all proportion to the small size of the sapphirine field. Thus, at temperatures up to 1460° C. almost two-thirds of the composition quadrilateral *CSAM* fails to give spinel as a final phase, and between 1460° C. and 1475° C. (fig. 6) the area free of spinel is large, although much less extensive. In contrast, the entire quadrilateral would yield some spinel accord-

ing to the earlier diagram. Furthermore, sapphirine will be present up to 1460° C. in all mixtures within the quadrilateral and up to 1475° C. in the greater portion of it, whereas its existence was not even recognized in the earlier diagram. Above 1475° C., of course, the conditions will be those indicated by figure 1.

It is not surprising that sapphirine was not recognized by Rankin and Merwin (1918) in their pioneer work on the system MgO-Al_2O_3-SiO_2. In the quenching method employed by them, liquidus temperatures of the various primary phases are given by the first appearance on cooling (or last disappearance on heating) of a few tiny crystals in a predominant glass. Under such conditions it is doubtful whether sapphirine could be distinguished conclusively from spinel. It will be recalled that great difficulty was encountered in the microscopic identification of phases in the calcines of the present study. To add to the difficulty, the field of primary crystallization of sapphirine must be far removed from its composition point, and it is almost certainly very small. It would be very easy, therefore, to miss the field of sapphirine because of the chance failure to study

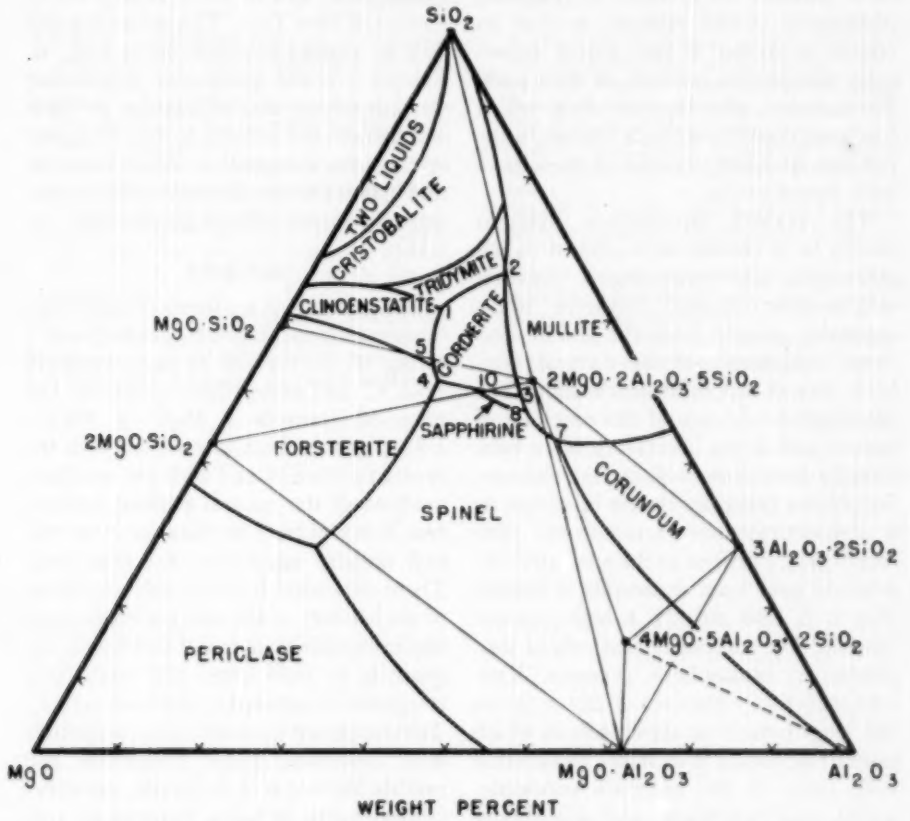


FIG. 8.—Tentative revised equilibrium diagram of the system MgO-Al_2O_3-SiO_2

mixtures falling therein. Probably not more than one of the compositions studied by Rankin and Merwin was so located as to be even potentially capable of giving sapphirine as the primary phase. Then, too, the primary field of sapphirine as tentatively outlined in figure 8 falls completely outside the limits of those compositions which yield sapphirine as a final consolidation product. This means that primary sapphirine has only a transitory existence on cooling. Natural sapphirine was less well known at the time of Rankin and Merwin's work, so that there was less likelihood of suspecting its presence. Indeed, they made no mention whatsoever of this mineral, so that its failure to appear in the system apparently aroused no concern on their part. Furthermore, the thermal data which first awakened the writer's interest in the problem were not available at the time of their investigation.

The revised equilibrium diagram should be of considerable interest to the petrologist and mineralogist. Another rock-forming mineral, hitherto unaccountably absent from the list of synthetic equivalents of the natural minerals, has at last been synthesized. The petrological evidence of the crucibles of nature and of the laboratory have been brought into more close correspondence. Sapphirine generally occurs in nature as a high-temperature metamorphic mineral (1944). Previous to the present study it would have been reasonable to believe that it is also strictly a high-pressure mineral. The successful synthesis of sapphirine at atmospheric pressure, however, effectively disposes of any such belief. Vogt (1947) has cited cases in which sapphirine occurs in intimate association with each of the minerals cordierite, spinel, and corundum, and accordingly his stability diagram indicates joins of sapphirine to each of these minerals. He was unable to decide whether the remaining join should be between cordierite and corundum or between sapphirine and sillimanite (which latter is generally encountered in nature in place of mullite). Since his relations, as far as they go, parallel those revealed in the 1450° C. calcines of the present study, it appears probable that the fourth join also is essentially the same in nature as in the laboratory and that it should be between sapphirine and sillimanite. Palache, Berman, and Frondel (1944) record the occurrence of sapphirine and sillimanite in Madagascar and in the Peekskill emery district of New York. This seems to point also to a sapphirine-sillimanite join, although it is not specifically stated that the sapphirine and sillimanite in these occurrences are intimately and mutually intergrown, a condition which must be met before the two minerals could be considered a stable mineral assemblage.

SUMMARY

The laboratory synthesis of sapphirine has been accomplished by prolonged sintering of the oxides at approximately 1450° C. and atmospheric pressure. The chemical formula $4\ MgO \cdot 5\ Al_2O_3 \cdot 2\ SiO_2$ appears best to conform with the synthetic mineral and with the available analyses of the natural mineral. Optical and X-ray diffraction data for synthetic and natural sapphirine are compared. These data furnish indisputable evidence of the identity of the two materials. Synthetic sapphirine is found to melt incongruently to yield spinel and liquid at a temperature believed to be about 1475° C. The compound is shown to be compatible with cordierite, spinel, corundum, and mullite but not with forsterite, periclase, clinoenstatite, or silica. Reasons are presented for the belief in the existence of a

primary field of crystallization of sapphirine on the liquidus surface of the MgO-Al_2O_3-SiO_2 system. A revised equilibrium diagram for the system is presented, and its tentative nature emphasized. The petrological significance of this revision is briefly discussed. Finally, the need for further work, in order to answer more conclusively the various questions which have arisen during this investigation, is stressed.

ACKNOWLEDGMENTS.—The writer wishes to express his gratitude to the officials of the Champion Spark Plug Company for permission to publish this paper. He is particularly grateful to Mr. F. H. Riddle, vice-president and factory manager, and to Mr. H. F. Royal, director of research, of the Ceramic Division, for their constant encouragement during the course of the investigation. He is deeply indebted to his former teacher, Dr. N. L. Bowen, who first introduced him to high-temperature silicate research and who critically reviewed the present manuscript. The assistance of Dr. J. F. Schairer, who contributed valuable thermal data, is also gratefully acknowledged. Dr. Herbert Insley, Mr. M. H. Barnes, and Dr. W. F. Foshag kindly supplied specimens of synthetic spinel and natural sapphirine.

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GEOLOGICAL NOTES

CURRENT-RIPPLE PROFILES AND THEIR DEVELOPMENT[1]

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INTRODUCTION

Water running over a granular bed often produces current ripples (ripple mark). If the velocity of the water remains constant, the shape of the current ripples varies with time until a stable form results. The shape also varies in response to a change in current velocity. The writer had opportunity to observe modifications in the transverse profiles of current ripples through the glass window in the side of a hydraulic flume. The flume was 6 feet long and 1 foot wide, and the current depth was 1–6 inches. The bed slope was 0.002 before the bed was rippled. Thus the hydraulic conditions in the flume were similar to those existing in small streams.

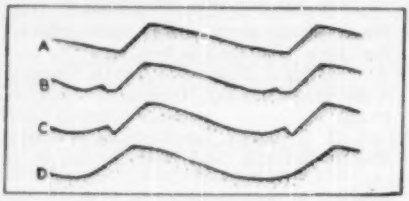

FIG. 1.—The development of the profile of current ripples if the water velocity remains constant. Current moving from right to left. *A*, initial angular profiles; *B*, eddy ripplets form; *C*, eddy ripplets and ripples join; and *D*, slumping reduces the dip of the lee slope.

CONSTANT VELOCITY

Small, rather angular ripples are produced if a shallow current, flowing over a bed of fine or medium sand, has a velocity just competent to move the bed. The lee slope of each of these ripples dips at an angle of about 41°–45°, although the precise angle depends on the angle of repose of the bed material. After a few minutes, the stoss slope is reshaped by an eddy which forms alee of each ripple. The eddy produces a tiny, ephemeral "ripplet," which moves *upstream* toward the lee slope of the preceding primary ripple. The lee slope of the ripplet has the same dip as that of the larger primary ripple. However, when the ripple and the ripplet meet, the base of the primary ripple slumps over the ripplet, and, as a consequence, the lee slope angle of the ripples is reduced to approximately 32°–37°. This sequence in the development of ripples is shown in figure 1.

DIMINISHED VELOCITY

The shape of fully developed ripples is modified if the current velocity is decreased. Figure 2 depicts two common types of altered ripples. In type *A* the grains which were at the crest of a ripple have been moved over the crest and have come to rest at a steep angle of repose. When a number of small ripplets are produced on a primary ripple, type *B* results. The ripplets do not always form on the crests of primary ripples; indeed, they usually appear in the troughs between ripples if the bed is composed of

FIG. 2.—Ripple profiles modified by a reduction in the current velocity. Current moving from right to left.

[1] Contribution No. 493 from the Woods Hole Oceanographic Institution. Manuscript received September 23, 1949.

uniform sand. If the bed material is not uniform in size (particularly if it is poorly sorted) the ripples form on the crests because the sand grains on the crests are finer than those in the troughs. This distribution of sizes is shown in figure 3. A slow current moving over such ripples (which were produced by a faster current) may be competent to move the fine grains but not the coarse ones. If the slow current velocity remains constant, the ripplets move over the crest of each primary ripple, and fine sand is deposited alee of the coarse base of the primary ripple (fig. 3, *B*). This results in a long flat ripple with a dip of about 41°–45° on the lee slope.

GEOLOGICAL SIGNIFICANCE

Although the ripple forms described in this paper may have some use in determining the tops from the bottoms of strata, they are perhaps more important as indicators of current velocities in ancient watercourses. The presence of eddy ripplets or of primary ripples with a lee-slope dip of more than about 40° would seem to indicate that the velocity of the currents which produced the ripples was but little more than the competent velocity for the sand size and that the current probably was not long enduring at that velocity. The modified ripple profiles shown in figures 2 and 3 are clear evidence that the current velocity had diminished when they were formed. Moreover, the size distribution illustrated in figure 3, *B*, indicates that the current was competent to move the small grains but not the large ones.

ACKNOWLEDGMENTS—The writer is indebted to Dr. R. Dana Russell for a critical reading of the manuscript.

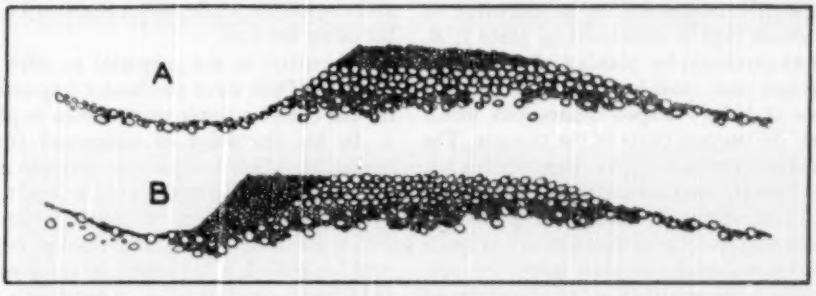

FIG. 3.—Distribution of grain sizes in ripples before and after reducing current velocity. Current moving from right to left.

SOME UNUSUAL RIPPLE MARKS FROM THE TRIASSIC OF UTAH[1]

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University of Utah

Ripple mark is one of the most commonly observed primary features of sedimentary rocks. It has been noted in rocks of practically all ages, and its various paleogeographical implications have been clarified by comparison with modern occurrences. Several detailed papers dealing with ripple marks have been written (Kindle, 1917; Bucher, 1919), and numerous writers have published illustrations and incidental comments in connection with broader investigations.

The purpose of the present paper is to place on record some unusual forms of ripple mark observed in the red silty facies of the Moenkopi formation near Temple Mountain, in the southeastern portion of the San

[1] Manuscript received October 17, 1949.

Rafael Swell, Emery County, Utah. Conditions during Moenkopi time were evidently especially favorable for the production and preservation of ripple mark, since this feature is present over very wide areas and through a vertical interval of several hundred feet. All lines of evidence indicate that the red-bed facies of the Moenkopi were laid down in very shallow water or on mud flats marginal to and only slightly above the general level of the early Triassic sea in which highly fossiliferous marine beds were laid down farther northwest in Utah and Idaho. The present examples of ripple mark were obtained about 150 feet below the upper contact of the Moenkopi formation.

Plate 1, *A*, illustrates a commonly occurring type which corresponds with what Bucher has termed "hexagonal interference-ripple mark." The cross-bars are slightly oblique to the main ridges and have a staggered arrangement. A modification of the above type is illustrated by plate 1, *B*. It was produced by planing off the crests of ridges and cross-bars so as to leave a series of kidney-shaped depressions which mark the deepest parts of the troughs. The depressions are arranged in regular rows but vary in depth and distinctness. Examples of the rock were excavated from a bedrock exposure, and other similarly marked specimens were found scattered below the outcrop, so it is evident that the truncation of the ridges did not occur through recent erosion. The markings occur in a layer of sandy siltstone about ½ inch thick, which is underlain by soft red shale. Above the ripple-marked layer is another shale lamina which is about ½ inch in thickness and is followed by a sandstone layer with ordinary ripple mark about 3 inches thick. The markings suggest some sort of organic imprint, but the evidence of production from and gradation into other types of ripple mark is clearly evident.

The specimen shown in plate 2 is the most complex example of ripple mark seen in the Moenkopi formation. Three components are evident: (1) prominent parallel ridges, (2) secondary lower parallel ridges, and (3) cross-bars. Kindle (1917, p. 105) has published an illustration of a specimen showing the low intermediate ridge but makes no explanation as to origin. The cross-bars of the present example are low and broad with distinctly rounded crests, whereas the indentations or furrows between them are relatively sharp and narrow. The secondary ridges seem to be made up of a series of separate, poorly defined coalescing segments and are, on the whole, much less sharp than the other features.

The writer is not prepared to offer an analysis of the wave mechanics responsible for the complex ripple mark shown in plate 2. In his discussion of compound ripple marks, to which the present example may belong, Bucher writes (1919, p. 195): "A systematic discussion of these forms to which the term 'compound ripples' might well be applied, is impossible at the present time, since practically no observations are available of the factors entering into their formation, or even of the forms themselves." It is hoped that the accompanying illustrations will furnish at least a few examples of the possible variations of form for some future analyst of the problem of ripple mark.

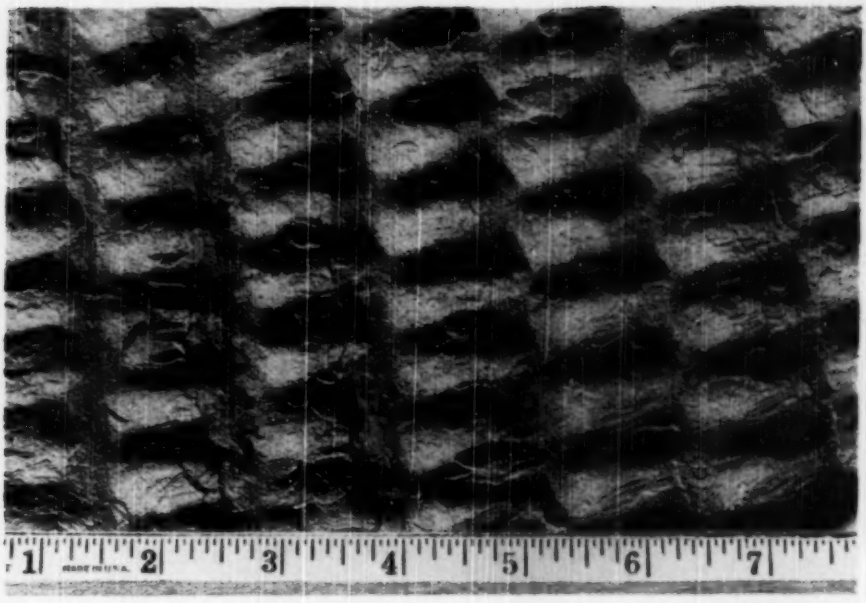
PLATE 1

A, Ordinary interference ripple mark from the Moenkopi formation, San Rafael Swell, Utah. Scale in inches.

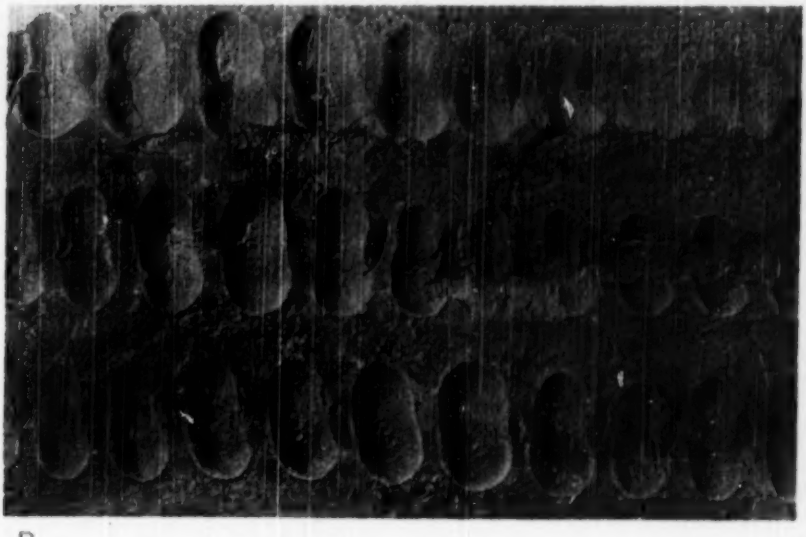
B, Truncated interference ripple mark from the Moenkopi formation, San Rafael Swell, Utah. Scale same as *A*.

PLATE 2

Complex ripple mark from the Moenkopi formation, San Rafael Swell, Utah. Scale same as plate 1, *A*.



A



B

Interference ripples

Interference ripples

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AN OBSERVATION BEARING ON THE PROBLEM OF EXFOLIATION[1]

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In 1925 Eliot Blackwelder presented evidence supporting the thesis that chemical, rather than temperature, changes are primarily responsible for exfoliation of rocks. In 1936 David T. Griggs offered experimental data in support of Blackwelder's thesis. Griggs subjected a specimen of granite to temperature changes of 110° C. or 198° F. in cycles of 15 minutes, each cycle being considered to represent 1 day. On this basis the number of cycles through which the specimen was run was computed to be the equivalent of 244 years. No exfoliation of the granite was observed. Objections to the experiment on the basis of the brevity of the temperature cycles are partially met by the following observations.

During the summer of 1949 the writer was introduced for the first time to the Sauna or Finnish steam bath, in which steam is generated by splashing water on highly heated pebbles. The heating unit in this instance consisted of a $\frac{3}{16}$-inch sheet-metal box, about 28 inches square and 31 inches high, with a centrally located firebox. The space around and above the firebox was filled with smoothly rounded pebbles ranging from about $1\frac{1}{2}$ to 4 inches in diameter. Most of the pebbles were of granitic types, but some gneisses, basic igneous rocks, and reddish sandstones were identified. Other types were undoubtedly present but could not be identified without breaking the pebbles.

This particular unit had been in operation every summer since 1937, this being its thirteenth year. Each day, for an average of 80 days each summer, or a total of about 1,000 days up to the time of these observations, the rocks surrounding and covering the firebox had been heated well above 212° F., as indicated by the fact that the water splashed on the pebbles was converted instantaneously into steam. The rocks were maintained at these high temperatures for several hours per day. Minimum temperatures during the summer were above freezing, but the steam room is unused and unheated during the winter, at which time temperatures as low as 50° F. below zero are recorded. Thus the pebbles, during the course of a year, are subjected to a far greater range of temperature than are those in nature.

This experiment, unintentional as it is, has been in progress for 13 years, and temperature changes are certainly the most important, if not the only, process affecting the rocks. In this regard it must be emphasized that no water is splashed on the pebbles until they are so hot that the water is immediately converted to steam. The water has no opportunity to penetrate the pebbles. Furthermore, the bulk of the water vapor in the room condenses on the walls and ceiling long before the pebbles are cool enough to serve as condensation surfaces.

About half the pebbles were removed for examination, a total of 516. Of these, 10 had split along flat planes, apparently original planes of weakness, and 41 were locally pitted as though some loosening of the mineral bonds had taken place. Not a single pebble, however, showed a trace of exfolia-

[1] Manuscript received October 31, 1949.

tion. Admitting the uncontrolled nature of the experiment, it would seem that great temperature changes, distributed as indicated over a period of 13 years, are insufficient to cause exfoliation. To that extent Blackwelder's thesis is supported.

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ON THE BIAS OF GRAIN-SIZE MEASUREMENTS MADE IN THIN SECTION[1]

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In studying the influence of grain size on variations in composition of thin sections, I have recently retraced a path first marked out by Dr. Krumbein (1935) and wish to suggest here that his principal result is both a little easier to reach and a little more difficult to interpret than one would gather from his excellent essay.

Krumbein points out that the trace of any grain dimension along a section of the grain will usually be shorter than the true dimension and that the mean of many such measurements is bound to be shorter than the mean value for the unsectioned grain. He then justifies, both empirically and analytically, the proposition that the proper estimate of a grain parameter, from measurements of the parameter as exposed in section, is $4/\pi$, or 1.27 times the average of the measurements.

Krumbein's proof is somewhat difficult to follow because it describes the general case, in which not only the measurements but the actual grain diameters vary.[2] For the purpose of eliminating the bias of the measurement, however, it makes no difference whether the dimension in question is regarded as constant for all grains or as varying from grain to grain. The first of these assumptions leads to a much simpler analysis.

In its simplest form, as in Krumbein's experiment with lead shot, the problem is to estimate the diameter of a circle from the lengths of chords passed through the circle at random. The probability of one chord's being longer than another chord of length m is the same as the probability that its distance from the center is less. If all distances from the center are taken as equally likely, this probability is h/r, where r is the radius of the circle and h is the distance of the first chord from the center. The element of frequency is dh/r, and, as $0 \leqslant h \leqslant r$, the total frequency is given by

$$\frac{1}{r}\int_0^r dh = 1 . \qquad (1)$$

But

$$h = \left(r^2 - \frac{m^2}{4}\right)^{1/2} = \tfrac{1}{2}\,(d^2 - m^2)^{1/2}, \qquad (2)$$

so that

$$dh = -\frac{1}{2}\,\frac{m\,dm}{(d^2 - m^2)^{1/2}}. \qquad (3)$$

The distribution function may be written

$$F = \frac{1}{r}\int_0^r dh = \frac{1}{d}\int_d^0 -\frac{m\,dm}{(d^2 - m^2)^{1/2}}, \qquad (4)$$

[1] Manuscript received December 2, 1949.

[2] Krumbein's proof also contains two typographical errors. On the next to the last line of p. 494, the "equal likelihood of cutting the section between y and $y+dy$" is given as $1/r$, but should be dy/r (the "element of frequency," dh/r, of the terminology used here). The substitution leading to a gamma function whose integration completes the proof (between eqs. [5] and [6], p. 496) is given as $x = \sin\theta$, but should be $x = r\sin\theta$. This substitution is not entirely necessary, however, because the function (eq. [5]) may be integrated by parts.

and the expected value of m, its first moment about zero, is

$$E(m) = \frac{1}{d}\int_d^0 -\frac{m^2 dm}{(d^2-m^2)^{1/2}} = \frac{\pi}{4}d. \quad (5)$$

Solving equation (5) for d and substituting the mean, $\bar{m}$, of the experimental measurements for the mathematical expectation, we have

$$d = \frac{4}{\pi}E(m) = 1.27\,\bar{m}, \quad (6)$$

which is Krumbein's result.

A similar result arises from a formula published without proof by Tomkeieff (1945). His formula for the average linear intercept of plane convex figures is

$$\bar{m} = \pi\frac{A}{P}, \quad (7)$$

where A is area and P perimeter. For the case of a circle this reduces to $d = (4/\pi)\bar{m}$, as before.

Despite the excellent agreement between observed and expected values obtained by Krumbein, there is no unique solution to the problem. There are, in fact, as many solutions as there are different procedures for selecting chords at random, and in statistics texts this is sometimes used as an illustration of the dictum that randomness resides not in the sample but in the sampling process (see, for instance, Aitken [1944], p. 11). If intercepts of circular sections are all measured along the same line and the sections are randomly oriented with regard to this line, then the angle between the chord and a tangent at its point of contact might have any value. This angle varies directly with the central angle subtended by the chord, and if all values of the central angle are taken as equally likely, then

$$\frac{1}{\pi}\int_0^\pi d\theta = 1. \quad (8)$$

Since

$$\theta = 2\sin^{-1}\frac{m}{d}, \quad (9)$$

$$d\theta = \frac{2\,dm}{(d^2-m^2)^{1/2}}; \quad (10)$$

so that

$$E(m) = \frac{2}{\pi}\int_0^d m\,(d^2-m^2)^{-1/2}dm = \frac{2d}{\pi} \quad (11)$$

and

$$d = \frac{\pi}{2}E(m) = 1.57\,\bar{m}, \quad (12)$$

which is quite different from equation (6) but just as valid in a purely formal sense.

If we substituted this procedure of measuring chords in an arbitrary direction for the diameter measurements used by Krumbein on the lead shot, we would have two experimental biases to eliminate: the first due to the uniform distribution of the distances of shot centers from the surface of the briquette and the second introduced by systematic underestimation of the diameter of circular sections of the shot. Denoting the shot (spherical) diameter by d, the diameter of the (circular) sections through the shot by d', and the average of the chord measurements by $\bar{m}$, we have

$$d = \frac{4}{\pi}d$$

and

$$d' = \frac{\pi}{2}\bar{m};$$

so that the proper estimate of the spherical diameter would be

$$d = \frac{4}{\pi}d' = 2\bar{m}$$

For the fairly common practice of measuring the projection of some dimension on an arbitrary reference line, a still different type of correction would be required.

Close agreement between observed and expected values may be anticipated only if the expected values are derived from a mathematical model whose assumptions of randomness are comparable to those of the experiment. From an observed agreement, however, we may not always infer the validity of the underlying assumptions; and in any case it is important to realize that the model is of the experimental procedure, not

of the material on which the experiment is performed. To illustrate this latter point, we may return to Krumbein's experimental data. His first step was to briquette some lead shot of known diameter. On the polished briquette he measured radii of circular sections of the spherical shot and found that the average radius of these circles was 0.763 per cent of the spherical radius. From equation (5) the theoretical value is $\pi/4$, or 0.785, so that the agreement is excellent.

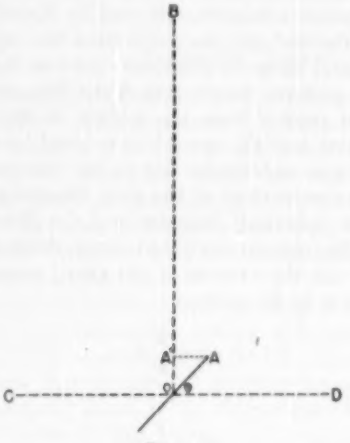

FIG. 1

The "grains" are spheres of very nearly the same diameter, and the radii of their circular sections are clearly determined by distances of (spherical) centers from the briquette surface. If we suppose that these distances are equally likely, the experiment is almost exactly mirrored by the mathematical model leading to equation (5). The experiment may be regarded as a test of the hypothesis that in the preparation of a briquette distances of grain centers from the surface are uniformly distributed.

In his second and third experiments Krumbein used an unconsolidated sand and an easily disaggregated sandstone. In neither case could it be assumed that the grains were of the same size; hence it was necessary to measure samples of them, both unmounted and as exposed in bakelite-impregnated thin sections. For the St. Peter sandstone the average maximum horizontal dimension of the mounted and unmounted grains was 0.149 and 0.193 mm., respectively; and the ratio of these values, 0.772, again agrees closely with the expected value of 0.785. A third sample, this time of a "slightly reworked glacial sand," was measured in the same way and yielded a ratio of 0.774. Results for all three samples are thus in remarkable agreement.

Krumbein finds this surprising in view of the obvious shape differences involved. He suggests that the controlling factor is sphericity rather than roundness; for, although the roundness of grains differs considerably as between the two sand samples, their sphericity indices are rather high and not very different. It is pretty clear on intuitive grounds that a good result may be expected if the grains are actually spherical and the distances of their centers from the surface of the briquette are uniformly distributed. The experiment with the lead shot confirms this expectation. It may be shown, however, that agreement between observed and expected values is independent of grain shape, however defined or measured.

In figure 1 a pointer of length d rotates in the plane of the paper about an axle normal to its midpoint, O, with angular velocity proportional to the sine of the angle AOD, so that the distance OA' varies uniformly in time. The apparent length of the pointer, e.g., its projection on CD, is observed intermittently at B.

If the schedule of measurements is independent of the period of the pointer, the average projection length will be $(\pi/4)d$ for the same reason that Krumbein's briquetted lead shot gave this result. If the angular velocity of the pointer were constant, however, the case would conform to the type of randomness on which equation (10) is based, and the average projection length would be $(2/\pi)d$.

Now the pointer is not in any sense a sphere or a circle, yet its length may be esti-

mated by the same procedure as that which Krumbein used on lead shot. It is true, of course, that in its rotation the pointer describes a circle, but even this is not necessary. A similar result will be obtained if the observation point rotates about O[3] while the pointer remains at rest and the projection normal to the line of vision is measured. In this case the pointer describes no circle at all, while the radius of the circle described by the observer is not given and cannot be found; yet the bias involved in estimating d from $\bar{m}$ may still be discovered if the angular velocity of the observer is adequately specified.

Evidently, the connection between model and experiment is less direct than inferred by Krumbein. Circularity (or sphericity) is to be regarded here not as a property of the grain but as a *description of the behavior of the grain parameter being measured*. This may seem like a quibble, but it is nothing of the sort. Instead of a rotating pointer, we might be concerned with the average c-axis of platy plagioclase crystals suspended in a basaltic lava quenched abruptly during turbulent flow. Or we might measure the a- and b-axes of elongate, rodlike hornblende crystals in a rock whose linear fabric was determined by the parallel alignment of the c-axes of these same amphibole crystals. In neither case would the shape of the grains have much to do with the matter.

The concordance of experimental and expected values also seems to be independent of whether the grains are all the same size or not. An assertion to this effect forms the only basis for suggesting that the simple proof offered here is an adequate substitute for Krumbein's, so some amplification of the point may serve as a useful summary. If d is constant, the moments of the distribution are given by integration of the distribution function,

$$E(m^n) = \frac{1}{d}\int_d^0 -\frac{m^{n+1}\,dm}{(d^2 - m^2)^{1/2}}, \quad (13)$$

[3] Anyone attempting this would be wise to use an automatic recorder.

for successively higher integral values of n, and the result is always a numerical coefficient times the diameter raised to the same power as the order of the moment. If the grain diameter is taken as variable but the nature of its variability is unspecified, the theoretical moments are those given by Krumbein, shown in table 1 under the heading "d Variable." Results for the two cases are compared in table 1. By far the most important of these quantities is the first moment, and evidently whether we regard the sample mean as a biased estimate of a fixed

TABLE 1

Sample Statistic	Theoretical Moment for	
	d Constant	d Variable
v_{m_1}	$\frac{\pi}{4}d$	$\frac{\pi}{4}v_{d_1}$
v_{m_2}	$\frac{2}{3}d^2$	$\frac{2}{3}v_{d_2}$
v_{m_3}	$\frac{3\pi}{16}d^3$	$\frac{3\pi}{16}v_{d_3}$
v_{m_4}	$\frac{8}{15}d^4$	$\frac{8}{15}v_{d_4}$

parameter or as a biased estimate of the mean value of a variable parameter makes no difference; in either case multiplication of the sample value by $4/\pi$ eliminates the bias. Similarly, an unbiased estimate of the second moment of the parent-population (of grains) is obtained through multiplication of the observed second moment by $3/2$, whether d be regarded as fixed or as variable.

The agreement between observed and expected results did not tell us anything about the shape of the grains and now, it seems, is equally uninformative about whether they vary in size. Finally, this agreement tells us how to adjust the observed value so that it is an unbiased measurement of size; but it tells us next to nothing about size itself, unless we are prepared either to define size as whatever comes out of the measuring proc-

ess or to relate the result of measurement to some previously agreed-upon definition.

The moral is by now clear; the bias of the measurement is not introduced by the grains and may be neither estimated nor eliminated by knowledge about them. It is introduced by the experimental technique, and a careful examination of the procedure may suggest computations leading to an unbiased estimate.

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GRAIN-SIZE MEASUREMENTS MADE IN THIN SECTION: COMMENTS[1]

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Dr. Chayes reopens a problem on grain-size analysis that has lain undeservedly dormant for several years, and he clears several points which have been in a very unsatisfactory state of development. At the time I wrote the original paper, it was my impression that no previous work had been done on the problem, but correspondence from several workers indicated that the subject had been attacked previously from several points of view.

Hagermann had studied the problem of embedded spheres as early as 1924, with a somewhat different mathematical approach but with similar results. Wicksell had used the essential features of my analysis in his study of globular star clusters in 1925, and in 1926 he applied a more rigorous treatment to the study of spherical bodies embedded in tissues. Fisher, in 1933, attacked the problem of grain-size analysis in indurated sediments from a viewpoint different from either Hagermann or myself. These developments are mentioned in Krumbein and Pettijohn, *Manual of Sedimentary Petrography* (1938), pp. 129–134, which includes the necessary references.

One of the features which emerged from consideration of Wicksell's work was that his more general theory on embedded bodies seemed to fit the experimental sedimentary data less well than did his simpler star-cluster theory. I assumed that this difference lay in the apparent fact that sedimentary grains are in surface contact, whereas bodies embedded in tissues may not be.

Chayes's current work shows that a relatively simple mathematical development yields the same results as even the intermediate theory based on variable grain diameters and variable grain measurements. His finding that the results are independent of grain shape is of particular importance; and apparently the results are partly independent of the size definition used, but do depend upon the experimental procedure. These features imply that definitions of "size" may require additional study and that computational procedures be developed along lines which yield unbiased estimates for the techniques used, as Chayes himself suggests.

Dr. Chayes is to be complimented on reopening interest in an important aspect of grain-size analysis. Certainly, the need for methods which permit comparison of particle size and shape from thin section with those of loose grains is even more pressing now than it was fifteen years ago.

[1] Manuscript received February 16, 1950.

DISCUSSION

SUBMARINE CANYONS AND THE ICE AGE: A DISCUSSION[1]

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As Shepard (1948) in his book *Submarine Geology* has provided a comprehensive summary of the available data concerning submarine canyons and of the hypotheses presented to explain their occurrence, it is unnecessary to review this material here. It should suffice to relate that such canyons appear to have a world-wide distribution and that their characteristics so closely resemble those of stream valleys produced by subaerial erosion that hypotheses which account for them by appeal to local factors (environmental, structural, or of process) are ruled out; and, short of inventing a universal submarine process capable of duplicating the effects produced by stream erosion, the conclusion is quite inescapable that subaerial stream erosion also carved the submarine canyons.

Despite valiant intellectual effort, no one has been able to invent a process of sea-floor erosion that could in any significant degree match the effects resulting from stream action. Accordingly, the obvious recourse is to attribute the submarine canyons to subaerial erosion. Then, however, the interpreter is confronted with the geologic horror of postulating either a universal rise of the lands to altitudes from a minimum of 6,000 to a maximum of 12,000 feet higher than present elevations above sea level or, equally unthinkable, a decline of sea level in like measure.

Moreover, as if such diastrophic or eustatic shifts were not a preposterous enough concept, when considered as a possible development over long geologic periods, the evidence is clear that the changes in level must have occurred since the Pliocene; for Pliocene sediments are transected by the canyon cuts. Submarine canyons, however created, were produced within the Pleistocene epoch. A weak evasion of this conclusion is afforded by contending that, though some submarine canyons are shown to be post-Pliocene, this does not prove that they all are. Except as such contenders then hope to find refuge from accepting a universal change in level of lands, in terms of thousands of feet, by appealing to narrowly localized factors in explanation of each canyon or group of canyons in turn, such by-passing of the issue is of little significance. And when the point is conceded, the evaders find themselves rather completely at a loss to formulate sets of conditions in any geologic age to give origin to such canyons.

The geologic impasse of recent, short-period, multithousand-foot sea-level changes has been falteringly approached. It is suggested that the Pleistocene ice sheets, instead of developing to maximal thicknesses of approximately 10,000 feet, may have attained a depth of 30,000 or perhaps 40,000 feet. Even if this assumption is made, the lowering of the sea level would be in terms of only 1,000 or so feet, by no means adequate to permit canyon-cutting to depths of 6,000 feet. Besides, what is known of the nature of glacial flow indicates that ice masses of such thickness at the centers of dispersion would have spread much more widely across the continental lands than the records show that they did. Further, evidence from existing glaciers and from along the margins of the Pleistocene ice rather definitely indicates, by slope gradients of the ice surface, that the ice sheets did not attain such thicknesses.

Another feeble suggestion is that a large part of the ocean waters were temporarily contained as vapor in the atmosphere. This is, at best, only a speculation which derives some support from modern knowledge that high temperatures prevail at very high levels in the atmosphere. Thus the potential absolute humidity of the outer atmosphere might be extremely great.

In terms of land uplift it has been proposed that the isostatic basining due to the piling-up of the Pleistocene ice sheets (especially if these

[1] Manuscript received October 6, 1949.

became 30,000 or more feet thick) would bring about a bowing-up of the continental margins around their borders. Then the sea bottom would be exposed, and steep slopes would be provided for rapid erosion of canyons. This is a plausible idea and does not violate accepted geologic doctrine. It has, however, the great defect that erosion so induced would be a localized phenomenon and completely inapplicable to a submarine canyon off the mouth of the Congo River.

In view of these difficulties, it is here suggested that the dilemma be resolved by accepting as a fact world-wide relative change in level of land and sea in the measure of thousands of feet in the Pleistocene.

The questions which then follow are: Why and how did such a shift take place? and In what degree do submarine canyons become explicable in view of such change? Incidental to these queries is the question: How do the phenomena of the Pleistocene fit into the picture?

A difference in altitude of the surface of the earth of 10,000 feet is a minor change in the configuration of a body that has a radius of 4,000 miles; a change of the magnitude of one two-thousandth. This is a point commonly made in discussing the height of mountains and the deeps of the ocean, both of which are several times as great. Further, the existing differences in surface level are regularly attributed to isostatic balance.

Suppose, then, that, instead of radio-heating (which is regularly adduced to develop isostatic unbalance by decreasing density), interior earth conditions are assumed to favor a temporary *increase* in the over-all density of the earth, or perhaps only in the postulated sima substratum of the crust. Two effects will follow. First, the ocean bottoms will sink to greater depths, and the capacity of their basins will be correspondingly increased. Accordingly, sea level will decline in a significant degree. Second, as the basic substratum, in which the sialic continents float, becomes denser, the continents will float higher. Such a rise would be vertical; it would affect all continents equally and simultaneously; it could be brought about with geologically extreme rapidity. The continents, indeed, may be thought of as popping up under such circumstances.

Inasmuch as all representations of interior earth conditions are, at best, highly speculative, there is no valid reason why increases in density of the sima shell should not be so nonchalantly postulated as are decreases. Further, as local or even continent-wide (Africa) epeirogenic uplift is a commonplace citation in paleogeographic, historico-geological interpretations, no qualms should be suffered when recourse is had to a universal continental uprise.

It should be recognized, further, that a comparatively slight increase in density of the sima would suffice to produce the increase in relief here proposed. At this point structural geologists would be inclined to introduce mathematics for evaluation of the necessary change. Except to provide an aura of profundity, such formulations could have little significance. The pertinent thing is that the measure of change produced would be that required to permit the erosion of the submarine canyons by subaerial processes. Computations of the density change required would be significant only in so far as they might serve to demonstrate that the degree of difference from the present would be very slight.

In shying away from universal vertical epeirogenic uplift, other elucidators of submarine canyon origin have been motivated by the realization that there is no inland evidence of stream rejuvenation through change in sea level in the measure of 6,000 feet or more. What is then overlooked is that, with vertical uplift, no acceleration of stream flow would occur at inland stations. Pre-existing interior slopes would remain unaltered. Only at the continental edges, over the continental shelves, and down the continental slopes would such rejuvenation be experienced. There, however, it would be terrific. There full-bodied streams would plunge over slopes that Shepard says are as steep as those of youthful mountain ranges. The observed canyon-cutting could be accomplished in a very short period. But the extension inland of the gorge trenches would be only so far as headwater erosion and accompanying knick-punkt retreat could bring it about. Inland from the heads of the submarine canyons streams would continue their placid pre-uplift courses, quite unaffected by the change in altitude of the land surfaces above sea level.

Not so with climatic regime. A 6,000-foot change in altitude of sea level would result in a marked refrigeration. By correlation with observed temperature differences with change in altitude, that measure of increase in elevation would bring about a reduction of 15°–20° F. in average annual temperature. This is rather precisely in accord with the amount of refrigeration

computed to have been in effect during the Pleistocene glacial stages. Moreover, Flint (1947) in his *Glacial Geology and the Pleistocene Epoch*, after reviewing all hypotheses of the cause of a glacial period, concludes that a combination of fluctuation in solar radiation and the presence of highlands is the best solution. This he calls the "solar-topographic hypothesis" (p. 512). Again, as with Shepard's account of the submarine canyons, the evidence which Flint advances in support of his conclusion need not be rehearsed here. It is noteworthy how well his summary of the data fits into the concept of universal continental uplift. Flint, further, is of the opinion that the successive ice advances and retreats were responses to diminishing and increasing heat supply. This interpretation permits retention of the continental heights throughout the Pleistocene for the promotion of submarine canyon erosion. However, once the validity of the major epeirogenic rise is conceded, there is little difficulty in recognizing the possibility that there were fluctuations in altitude during the epoch; these may even have been rhythmically ordered by alternations in the intensity of the densification factor, whatever its nature. In other words, one need not rely solely upon differences in the amount of solar energy received to account for the several advances and retreats of the Pleistocene continental glacierization.

Shepard calls attention to several circumstances that would tend to invalidate this hypothesis of universal epeirogenic continental uplift and accompanying sinking of the sea bottom. One is that the Mediterranean Basin and the Japanese Sea would be converted to lakes, and, except as their levels were lowered by evaporation, there would be no possibility for subaerial erosion of the mile-deep canyons on their underwater slopes. Another is that oceanic islands show no evidence of having been carried down 6,000 feet by the universal lowering of the sea bottom by that amount. This objection is partly met by reference to the occurrence of flat-topped, apparently wave-truncated, seamounts (guyots) at depths of between 4,000 and 5,000 feet. If part of the altitude change resulted from uplift of lands, the islands may have participated in this, in accordance with the degree of their isostatic balance, and thus have escaped submergence. Some, at the level established by their measure of going down with the bottom and their own compensating uplift, were truncated. Then, when the present level was reestablished, they became guyots. Or, as Shepard points out, the guyots may be relics of sea-level changes or local sinking of the sea bottom, far antedating the Pleistocene. As a final point, it should be noted that Shepard concludes that the continental slopes are best accounted for as the reflection of high-angle faults.

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L'OXYGÈNE EN LES CALCULS PÉTROGRAPHIQUES: UNE DISCUSSION[1]

RENÉ PERRIN[2]

ENGLISH SUMMARY

Ivan Th. Rosenqvist (1949) stated that "the diffusion coefficients in rocks are too low to cause any mass transport in the solid state over a great distance" and calculated that it would require a million years for 0.3 mg. of an element to diffuse through 1 cm.[2] He based his judgment on the values of the diffusion of coefficients of ions in a solid as determined by Bowen, Niggli, Brandenberger, and himself. Observations made in the course of refractory work had caused the present author (1934*a*, *b*) to attribute metamorphism (metasomatism) and the origin of granites (1935) to solid diffusion. To recall the results of three types of experiments: (1) An example of actual metamorphism in a furnace lining composed of magnesia and lime. Here it was found that to a depth of 1 cm., where the temperature was about 1000°C., the TiO_2 content increased in 35 minutes from 0 to 10.8 per cent, while the lime content decreased from 14 to 4.4 per cent. This diffusion in two directions, comparable to metasomatism, took place without any infiltration of liquid

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into the lining. (2) The numerous examples of industrial reactions between solids, in which it is certain that silicates took part, were affected in short periods of time. With these in mind the probable results of similar diffusion speeds in the lithosphere in a million years are considered. Admitting that the phenomena would be infinitely slower because of the low temperatures and great distances, Rosenqvist's calculations are not disturbing because observations are preferable to speculations as a basis for theories.

Roubault and Perrin (1939) drew attention to "corrosion between crystals," solid into solid, in certain granites. Similar observations have been made on granites and metamorphic rocks in studies such as that of the Singo granite by King (1947), who concluded that it "evolved by a series of replacements" which left a record clearly readable in thin sections. It is pointed out in this paper that evidence that exchanges of such cations as Si, Al, Na, Mg, Fe, and Ca are responsible for metasomatism and granitization, is substantiated by an ever increasing number of observations. Although oxygen ions are also mobile, they are here considered statistically stationary, following Barth, for their enormous preponderance in weight and volume suggests that they do not have a reason to diffuse statistically. This appears to be the most satisfactory basic hypothesis, and it is reasonable to adopt it, at least provisionally. If the deductions which can be made from it are in accord with the facts, the hypothesis will progressively gain certainty; if the facts prove contrary to it, it should be abandoned.

In the course of the processes of metamorphism and granitization, a rock will expand, contract, or remain constant in volume, depending on whether the final rock contains less, more, or the same number of oxygen ions as the original rock. Following the work of Brajnikov on the concentration of oxygen in minerals and rocks, this author (1949) showed that, generally speaking, there should be an expansion, since the oxygen concentrations of detrital rocks are higher than those of plutonic rocks. It follows that metamorphism and granitization are important factors in orogeny. This author also stated in 1949 (in Barth, 1949*b*) that such expansion could be of the order of 8 per cent, a figure which corresponds roughly to the mean difference in oxygen concentration between detrital rocks and granite but would vary greatly, depending on the mineral content of the initial and final rocks. The principle of expansion is important, not the actual figure.

Since certain granite massifs do not appear to have displaced sedimentary or metamorphic rock strata in their midst and other massifs have given rise to cupolas and domes, it appears that the "volume law" is only approximate. Figures dealing with diverse cases show this; ideally, these cases should be ones where the initial rock and the final rock and, if possible, the intermediate stages are known; precise analyses and density measurements of rocks permit calculation of the variations in oxygen concentration during their transformation. A set of such data compiled by G. H. Anderson (1937) enabled calculations of volume increase of 4.1 per cent from metamorphosed phyllite to the granite which has replaced it; of 3 per cent from a biotite schist to a schist further transformed; and no volume increase to an almost granitized schist. From Grout's (1937) figures the author has calculated volume increases of 0.6 per cent for the country rocks of the Giant Range, 2.3 per cent from the rocks of the Boulder batholith, and 2.0 per cent for the Duluth gabbro. Ellis (1947, p. 148) has calculated a change in oxygen concentration ranging from 3 to 12 per cent during the formation of the Marievale granite by replacement in the solid state. The extreme figures for oxygen concentration in the rocks that Anderson studied differ by only 9 per cent. Using Brajnikov's data, it is established that (1) the oxygen concentrations of minerals vary according to the ratio 1.7:1, (2) those of detrital rocks are, on the whole, relatively high and variable, (3) the differences are small in the common metamorphic rocks, (4) those of plutonic rocks vary little, and (5) those of lavas have a net difference from others in their families of similar composition: there is, for example, 8 per cent deviation between rhyolite and granite. This does not appear to be by chance, but, following Le Châtelier's law, volume increases produced by chemical diffusion are opposed by the difficulty of deforming the whole of the rock. This inhibits the reactions, and the law of small volume changes is the natural consequence. The final result is a compromise between the tendency toward a physical-chemical equilibrium (in the whole of a terrane originally out of equilibrium) and the difficulty of deformation, which opposes the establishment of equilibrium.

What is the motive power of such chemical diffusions? At a given temperature and pressure the activity of oxygen ought to be the same for the different phase minerals if there is equilibrium. Detrital terranes, resulting from alteration and erosion, are not in equilibrium; but, if temperature and pressure become sufficient (if, for instance, these rocks are buried), reactions should take place so that the activity of oxygen in the different minerals becomes the same. But variation in the activity of oxygen as a function of temperature and pressure is not the same for different minerals. Under these conditions the re-establishment of equilibrium between different depths of the terrane, which are at unequal temperatures, should begin.

The constant development of crystallization, corrosion between crystals, the presence of inclusions and "relicts" of minerals, myrmekites, etc., show that equilibrium has not been completely attained in the majority of metamorphic and plutonic rocks. What one sees is the result of an uncompleted reaction, and it is the observation of the progressing reaction that permits discernment of the mechanism. The belief is here stated that the driving force is the tendency toward equilibrium between the activities of different bodies of different minerals, which acts to change the original chemical composition of terranes out of equilibrium within themselves and with deeper terranes. While this tendency toward equilibrium produces displacements of isotherms, variations of volume, and deformations, the difficulty of deforming a whole area limits chemical diffusion. Since the factors are extremely complex and varying in time, it is not astonishing that the results are also complex. Granite batholiths of homogeneous appearance are undoubtedly examples of close approach to this equilibrium. Yet detailed studies, like those of King, show the kinetics clearly in evidence, inscribed in the crystals of the rocks.

The author believes that the recent work on solid diffusion applied to petrology and the increasingly precise and numerous observations of details pertinent to the facts of granitization are building a new petrography, in agreement with the facts, and where certain laws are beginning to appear. It appears to be in

entire opposition to the magmatic theories, and it does not seem possible to reconcile the two theories by conceiving of a petrography taking part from each; for then one must admit that identical structures can be formed by radically opposed processes.

Warm tribute is paid to Tom F. W. Barth for the fundamental and fertile contributions made in his studies (1949*a*, *b*), in the belief that these contributions together, with those of Brajnikov, will greatly aid the progress of knowledge.

Dans sa discussion des articles de Tom F. W. Barth (1949*a*, *b*), Ivan Th. Rosenqvist (1949) affirme, «In any case, the diffusion coefficients in rocks are too low to cause any mass transport in the solid state over a great distance.» Il base son jugement sur les évaluations des coefficients de diffusions d'ions dans le solide, faites en particulier par Bowen, Niggli et Brandenberger et lui-même.

A la suite d'observations sur les réfractaires en métallurgie et sur les roches, j'ai, comme l'a rappelé Barth (1949*b*), attribué en 1934 le métamorphisme (métasomatisme), puis en 1935 la genèse des granites, aux diffusions dans le solide. Je devrais donc être très troublé par les calculs de Rosenqvist, car celui-ci dit qu'il faudrait un million d'années pour que 0.03 milligramme d'un élément diffuse à travers une section de un centimètre carré.

Mais je me souviens

1. De mes expériences de véritable métamorphisme artificiel sur des revêtements de four. Je citerai un exemple: dans un revêtement composé de magnésie et de chaux, j'ai constaté que, à une profondeur de un centimètre, où la température devait être d'un ordre de grandeur de 1000° centigrades, le contenu en TiO_2, initialement nul, est monté a 10,8 pour cent, tandis que le contenu en chaux baissait parallèlement de 14 pour cent à 4,4 pour cent (diffusions a double sens, semblables au métasomatisme). Ce résultat a été obtenu en trente-cinq minutes sans qu'il y ait la plus petite infiltration de liquide dans le revêtement.

2. Des nombreuses réactions entre solides réalisées industriellement en des durées courtes et dont certaines font intervenir des silicates.

3. Des synthèses faites en laboratoire.

Me rappelant ces faites expérimentaux, je songe aux résultats qu'auraient donné dans la lithosphère de pareilles vitesses de diffusion (ou réaction) en un million d'années; je pense que, du fait des températures plus basses et des grandes distances, les phénomènes ont été infiniment plus lents, mais je ne suis plus troublé par les affirmations de Rosenqvist; je préfère les résultats d'expérience aux calculs.

De même en pétrographie, les faits d'observation doivent avoir le dessus sur les spéculations et servir de base aux théories . C'est uniquement à partir de tels faits indiscutables que j'ai conclu à la genèse des granites dans le solide.

Entre nombre d'autres faits, nous avons, M. Roubault et moi (1939), attiré l'attention sur les phénomènes de "corrosion entre cristaux," solide par solide, observés par nous dans plusieurs granites. Des observations similaires se sont multipliées depuis, tant dans les granites que dans les roches métamorphiques; je citerai en particulier la remarquable étude du grand Singo batholith de granite (éruptif dans des schistes et quartzites et sans xenoliths) faite par Basil Charles King (1947). L'auteur conclut que le granit "evolved by a series of replacements." On peut dire que les diffusions successives dans le solide se lisent clairement dans la série des nombreuses slices minutieusement décrites par l'auteur; tous les calculs de Rosenqvist, basés sur des données plus qu'incertaines, ne peuvent venir à l'encontre de ce fait du Singo batholith et de beaucoup d'autres. Je suis convaincu qu'il est aisé de démontrer par des calculs que les roches ne peuvent pas, sans se briser, être plissées violemment. Et cependant elles sont.

Le nombre, qui grandit tous les jours, de faits prouvant l'importance majeure en pétrographie des diffusions dans le solide, m'autorise, je crois, à on parler et à fair quelques remarques au sujet des idées exprimées par Barth; celui-ci a publié d'ailleurs, avec mon accord, un extrait d'une lettre que je lui avais adressée (1949*b*).

Les observations de métasomatisme, granitisation, évolution des enclaves, prouvent à l'évidence que des échanges de tous les cations, Si, Al, K, Na, Mg, Fe, Ca, etc., se produisent. Les ions d'oxygène sont-ils eux aussi mobiles ou peut-on penser qu'ils restent stationnaires "statistically" comme le dit fort bien Barth. Cette supposition de stationnement statistique apparaît très probable, car l'énorme prépondérance en poids et plus encore en volume des ions d'oxygène qui forment la trame des roches peut faire raisonnablement penser que ces ions n'ont "statistically" aucune raison de diffuser. Dans tout les cas, c'est l'hypothèse de départ la plus satisfaisante, et c'est déjà une grande chose en pétrographie que d'avoir une hypothèse de dé-

part satisfaisante. Il est donc raisonnable de l'adopter, au moins provisoirement; si, par la suite, les déductions qui pouvent être tirées de l'hypothèse s'accordent avec les faits, l'hypothèse deviendra progressivement certitude; si les faits sont contraires, elle devra être abandonée.

Admettons donc avec Barth que l'oxygène reste statistiquement stationnaire, comme l'ont fait parallèlement H. H. Read et Lapadu-Hargues; il en résulte qu'au cours du processus de métamorphisme, puis de granitisation, la roche se dilatera, se contractera ou gardera un volume constant, selon que la roche finale contiendra, par centimètre cube, un nombre d'ions d'oxygène plus petit, plus grand ou le même que le roche de départ. Utilisant les travaux de Brajnikov sur la concentration en oxygène des minéraux et des roches, j'ai montré (1949) que, dans l'ensemble il devait y avoir eu dilatation, les concentrations en oxygène des roches détritiques étant plus fortes que celles des granites et roches plutoniques.

En conséquence, métamorphisme et granitisation sont un facteur dans l'orogenèse, facteur qui peut être important si les diffusions se font jusqu'aux couches assez profondes de la lithosphère, ce qui est probable.

Dans ma lettre à Barth, j'ai dit que cette dilatation pouvait être de l'ordre de huit pour cent. Il ne s'agit que d'une estimation, à laquelle je n'attache pas une importance spéciale; c'est le principe de la dilatation que me paraît important. Ce chiffre correspond à peu près à la différence moyenne de concentration en oxygène entre roches détritiques et granites; il est égal en particulier à la difference entre quartzites purs et granites moyens. Il doit naturellement varier beaucoup d'un cas à l'autre, selon la composition minéralogique de la roche de départ et la roche finale. L'observation prouve effectivement que certains massifs granitiques ne paraissent pas avoir déplacé les strates des roches sédimentaires ou métamorphiques dont certaines (quartzites en particulier) se prolongent dans leur sein, tandis que d'autres massifs ont donné naissance à des coupoles, des dômes, etc.

Ainsi la "volume law" ne me paraît être qu'approximative, ce que Barth indique aussi d'ailleurs. Maix, le mieux pour voir clair, est d'observer, dans des cas particuliers, où l'on connaît la roche de départ, la roche finale, granite ou granodiorite, par exemple, et si possible, des stades intermédiaires. La connaissance des analyses précises des roches et de leur densité permet de calculer la variation de concentration en oxygène lors de la transformation.

En attendant que soient faites dans ce but des études, très souhaitables, il m'a paru intéressant d'utiliser certains documents dont j'avais connaissance. Il y est donné, ce qui est rare, les densités en même temps que les analyses. Je me suis donc reporté d'abord à deux articles, riches en belles observations, l'un de G. H. Anderson (1937), l'autre de F. F. Grout (1937).

En utilisant les données d'Anderson, j'ai calculé:

1. Une augmentation de volume de 4,1 pour cent du phyllite métamorphique au granite de Pellissier qui l'a remplacé.

2. Une augmentation de trois pour cent d'un schiste à biotite à un schiste plus transformé, mais par contre nulle pour le schiste presque granitisé.

Grout donne des chiffres sur la précision desquels il insiste, relatifs à des roches, extérieures au pluton, et aux enclaves transformées provenant de ces roches, ce qui ne permet de mesurer qu'une partie de la variation de volume due à la granitisation totale. J'ai calculé des augmentations de volume de:

0,6 pour cent pour les enclaves du Giant Range
2,3 pour cent pour celles du Boulder batholith
2,0 pour cent pour le Duluth gabbro

Enfin, J. Ellis (1947, p. 148) a calculé des variations allant de trois à douze pour cent de concentrations d'oxygène lors de la formation, par remplacement dans le solide, du Marievale granophyre.

Ces chiffres, qui concernent des cas très divers, montrent que la "volume law" n'est certainement qu'approchée.[3]

On constate, en outre, d'après les données d'Anderson que les chiffres extrêmes de concentration en oxygène des différentes roches qu'il a étudiées diffèrent de neuf pour cent seulement.

Ainsi, si l'on utilise en outre les données indiquées par B. Brajnikov, on constate:

1. Que les concentrations en oxygène des minéraux naturels varient dans de fortes proportions de 1 à 1,7 environ.

2. Que celles des roches détritiques sont, dans l'ensemble, relativement élevées et variables.

[3] Il est des cas où la granitisation doit s'accompagner d'une diminution de volume; exemple: lors de la granitisation de roches laviques solidifiées, leur concentration en oxygène étant inférieure à celle du granite.

3. Que les différences sont déjà relativement faibles dans les roches métamorphiques communes.

4. Que les concentrations des roches plutoniques, des gabbros aux granites, varient peu. B. Brajnikov écrit, "dans les roches intrusives, le nombre d'atomes d'oxygène contenu dans un volume donné, est à peu près constant" et il remarque qu'il s'établit pour arriver à cette constance, une compensation entre les concentrations faibles de certains minéraux: feldspaths et micas noirs, et les concentrations plus fortes des amphiboles, micas blancs, quartz, etc.

5. Que les concentrations en oxygène des laves sont nettement différentes de celles des roches plutoniques de leur famille, d'analyse semblable: huit pour cent d'écart, par exemple, entre rhyolite et granite; le basalte a une concentration plus faible que le granite, à l'inverse du gabbro, etc.

Tout ceci est-il l'effet du hasard? Chacun a le droit de le croire, mais je ne le pense pas. Mon opinion est la suivante: en vertu du principle de Le Châtelier, rappelé par Rosenqvist, le fait que des diffusions chimiques produisent des augmentations de volume entraîne une réciproque: la difficulté de déformation des ensembles de roches s'oppose à la propagation de ces réactions et surtout à ce que s'accomplissent intégralement des réactions produisant des augmentations de volume très importants; la loi des faibles variations de volume est la conséquence naturelle de cette opposition. Par exemple, des terrains argileux contiennent bien assez d'alumine pour être transformés sous l'action de diffusions d'ions alcalins, en un massif de feldspaths seuls, mais ceci produirait une grande dilatation de quinze pour cent environ et des déformations considérables: aussi l'observation prouve que, dans la gneissification et la granitisation, il y a départ d'alumine et que la forte concentration en oxygène des grains de quartz vient compenser partiellement la faible concentration de feldspaths, etc.

Naturellement ceci est vrai dans l'ensemble, "statistically"; localement, dans des parties de roches en dilatation lors des efforts mécaniques, des concentrations de feldspaths peuvent se former.[4]

[4] La feldspathisation est donc plus facile, à température égale, dans les parties relativement superficielles plus déformables que dans les parties profondes; il ne m'est pas évident que les granites se prolongent tous tels quels avec leur structure visible à des profondeurs considérables.

Au total, le résultat final des transformations résulte d'un compromis entre une action: tendance vers un équilibre physico-chimique entre des ensembles de terrains initialement hors d'équilibre, et une réaction: difficulté de déformation s'opposant à l'établissement de cet équilibre.

Nous avons essayé, en partant de ces idées, de tenter, M. Roubault et moi (1949), un commencement d'interprétation de certains faits géologiques restés, a notre avis, inexpliqués.

Il nous semble que la concordance avec les faits est satisfaisante.

Mais, quel est le moteur de l'action, c'est-à-dire des diffusions chimiques? Barth remarque justement qu'à pression et température déterminées, l'activité de l'oxygène doit être la même pour les différentes phases minérales, s'il y a équilibre.

Dans des terrains détritiques, dont les éléments résultent d'altérations et d'érosions, il n'y a pas équilibre; si la température et la pression deviennent suffisantes, par exemple du fait de l'enfouissement en profondeur, des réactions doivent prendre place pour que l'activité de l'oxygène des différents minéraux nouveaux soit la même.

Mais en outre, la variation d'activité de l'oxygène en fonction de la température n'est pas la même pour les différents minéraux (de même qu'en fonction de la pression, selon les causes invoquées par Ramberg et Barth).

Dans ces conditions, des diffusions doivent s'effectuer dans un sens de rétablissement de l'équilibre entre les différentes couches de terrains à température inégale. Ce qui est vrai de l'oxygène l'est aussi des autres corps.

Les faits observés dans les roches métamorphiques et plutoniques: évolution incessante de la cristallisation, corrosion entre cristaux, existences d'enclaves et de "relicts" de minéraux, myrmékites, etc., montrent que cet équilibre n'a pas été complètement atteint dans la majorité des cas. Ce que l'on voit est le résultat d'une cinétique inachevée, fort heureusement d'ailleurs, car c'est l'observation de la cinétique qui permet de discerner les mécanismes.

Au total, je résumerai ainsi l'ensemble du mécanisme. Le moteur est la tendance vers un équilibre entre les activités de différents corps des différents minéraux, équilibre lui-même fonction de la température et de la pression; cette tendance agit en partant de compositions chimiques données de terrains hors d'équilibre en eux-mêmes et avec les terrains plus profonds.

Lors de cette tendance vers l'équilibre se produisent des déplacements d'isothermes, de fronts, des variations de volume et des déformations, mais la difficulté de ces déformations d'ensemble de terrains s'oppose aux diffusions chimiques ou tout au moins en limite la nature.

Ce que nous voyons est le résultat inachevé de la cinétique de cette interaction de causes extrêmement complexes et variant dans le temps; il n'est pas étonnant que souvent ce résultat soit lui-même extrêmement complexe: un exemple est la granite de Bénodet (Bretagne) décrit par R. Perrin et M. Roubault (1948).

Les batholithes granitiques d'allure homogène représentent l'exemple sans doute le plus approché que nous puissions voir de cet équilibre. Il est remarquable cependant que, dans certains de ces cas, des études minutieuses comme celle de King, mettent clairement en évidence la cinétique, inscrite dans les cristaux des roches. Cette évolution constante de la cristallisation apparaît d'ailleurs comme un phénomène d'ordre très général dans les roches métamorphiques et plutoniques.

Je terminerai, en disant qu'il me semble que, grâce aux travaux qui ne datent cependant que de peu d'années encore sur l'application à la pétrographie des diffusions dans le solide, et grâce, parallèlement aux observations de détail de plus en plus précises, nombreuses et convergentes sur les faits de granitisation, de genèse de filons, etc., une pétrographie nouvelle, cohérente avec les faits, et où certaines lois commencent à apparaître, s'édifie progressivement. Elle est, qu'on le veuille ou non, en opposition d'ensemble avec les théories magmatistes, malgré l'effort que font certains pour les concilier et concevoir une pétrographie faisant sa part à chaque théorie. Ces derniers sont contraints d'admettre, pour ce faire, que des structures identiques de roches peuvent avoir été engendrées par des processus radicalement opposés, et d'établir des coupures de modes de genèse là où l'observation ne montre que du continu, ça à quoi je ne puis me résoudre.

Je pense, enfin, qu'il faut rendre un vif hommage, même si l'on ne partage pas toutes ses opinions, à Tom F. W. Barth pour les contributions fondamentales et fertiles qu'il a apportées par ses études sur la "Distribution of oxygen in the lithosphere" et "The use of oxygen as a reference substance in petrographic calculations."

Je suis convaincu qu'elles contribueront fortement aux progrès dans la connaissance; jointes à celles de B. Brajnikov, elles m'ont, en tout cas, appris beaucoup de choses.

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TURBIDITY CURRENTS AND GRAYWACKES—A DISCUSSION[1]

F. J. PETTIJOHN
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The role of turbidity currents in sedimentation has not been understood and has been, in general, unappreciated. The paper "Turbidity Currents as a Cause of Graded Bedding," by Ph. H. Kuenen and C. I. Migliorini in this issue of the *Journal*, is therefore a most significant contribution to the study of the sedimentary deposits. Though turbidity currents have been known for some years, nobody seems to have examined the deposits made by these currents. Kuenen and Migliorini have done just this and have shown that these deposits have unique textures and structures by means of which they can be identified.

Students of modern marine sediments should now re-examine carefully the coarser materials collected in their coring tubes. Graded beds are known to occur in some cores and have even been suspected of being produced by turbidity currents (Bradley, 1940). That such deposits are common in some places seems highly probable. It may even be possible to determine from suitable cores whether the delta-like deposits at the mouths of some of the submarine canyons consist of truly fluviatile sediments or are the deposits of turbidity currents.

It is even more important, now that the diagnostic features of the deposits made by turbidity currents are known, that the rocks of the geologic record be re-examined to determine whether or not they include such deposits. Such rocks, if they exist, will demonstrate the reality and measure the importance of turbidity currents as agents of sediment transfer in the geologic past. Until this is done, one cannot say that turbidity currents are nonexistent or unimportant. The observations of Migliorini are, therefore, most significant.

Although the paper by Kuenen and Migliorini is concerned primarily with the problem of graded bedding, it sheds much light on the origin of the graywackes.[2] In 1927 I began my studies of the Archean graywackes and slates of the Canadian shield. Ever since then I have tried without success to find a graywacke in the process of formation. Study of graywackes soon made it clear that these rocks belong to no particular age and that those of the Archean and the Tertiary are identical in every particular. The failure to find a modern graywacke could not be attributed, therefore, to a nonrecurrent pre-Cambrian environment. The failure to find a modern graywacke is most astonishing, inasmuch as graywackes are a very common type of sandstone. Though found only in the folded geosynclinal belts, they form 15–20 per cent of all sandstones.[3] If graywackes are the product of submarine turbidity currents and are deposited in deep water, then we have an adequate explanation of our failure to date to locate any in the process of deposition.

Graywackes have many peculiarities which show that they are not the products of normal sedimentation. Their graded structure; their chaotic appearance under the microscope, with scattered angular sand grains in a prominent to dominant "clay" matrix; their lack of current bedding and ripple mark; the interbedding of the graywackes with radiolarian cherts; the numerous structures, both folds and faults, pro-

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[1] Manuscript received December 22, 1949.

[2] Kuenen and Migliorini designate some of the graded bedded sandstones as graywackes. Their description, however, and the close correlation between graded bedding and the graywacke type of sandstone leave little doubt that the bulk of the graded sandstones are true graywackes.

Several years ago T. G. Payne, of the U.S. Geological Survey, expressed the view that turbidity currents might be important in the distribution and production of the graywackes. This view, stated in conversation with the writer was, as far as I am aware, never published or elaborated on by Dr. Payne.

[3] Tallman (1949, p. 586) estimates 17 per cent. Graywackes are not to be confused with subgraywackes, which are even more abundant. Although the latter have been included by some in the graywacke class (see, e.g., Krynine, 1950), they are distinctly different from graywackes. I once thought the chief difference was the high feldspar content of the graywacke (1949, p. 255). It is clear from the work of Kuenen and Migliorini that a more important difference is the texture. The graywackes are marked by a primary mud matrix and are graded by reason of their deposition from a turbidity current, whereas the subgraywackes are characterized by an introduced mineral cement and are deposited from normal subaqueous currents.

duced by soft-sediment deformation; the many large and small fragments of shale; and the remarkable uniformity of thickness of the individual beds which is incompatible with their coarse texture—all these characteristics set graywackes apart from the normal lenticular, current-bedded sandstones with their interstitial mineral cement. In 1930 Bailey came to the conclusion that graywackes, unlike normal sandstones, were deep-water deposits. But it was not clear then just how deep-water deposition of the coarse grits was brought about.

Turbidity current transfer of coarse materials down very gentle slopes explains why coarse graywacke grits can accumulate in deep water. The observations of Kuenen and Migliorini leave little doubt of the reality of the process and remove an important mental block in our thinking. The prevalent and erroneous view that coarse clastics were indicative of shallow water required vast oscillations of the sea floor to deposit alternately coarse grits and radiolarian chert (such as are found in some sections). As pointed out by Migliorini, this unseemly behavior of Mother Earth is now quite unnecessary.

TABLE 1

TURBIDITY AND RELATED CURRENTS

Composition	Water and Mud		Gases and Volcanic Ejecta (Aerosol)
	Concentrated	Dilute (Hydrosol)	
Type.....	Mudflows	Turbidity currents	*Nuées ardentes*
Product...	Pseudo-tillite ("breciola")	Graywacke ("macigno")	Volcanic tuff ("ignimbrites")

Kuenen and Migliorini point out that associated with turbidity currents are submarine mudflows. The latter may even initiate a turbidity current. The mudflow deposits are, however, unlike those of the turbidity currents which they generate. They are essentially unstratified. These are disordered deposits characterized by sporadic boulders in a predominant clay matrix. They are indeed pseudo-tillites. The association of conglomerates of this type with the graywacke suite has long been an enigma. It seems highly probable that some of these conglomerates, such as these found in the Johns Valley shale of Oklahoma, are products of submarine slump.

It should be added that, if Kuenen and Migliorini are right—and I think they are—turbidity currents as distributors of sediment are exceedingly important. Not only are they and their associated mudflows responsible for a large and important group of sediments, but they are also important agents of submarine denudation. Though this has been suspected by some and advocated strongly by others, not until now have they had the evidence from the rocks of the actuality and magnitude of this action. The theory that the graded graywackes and mudstones are not the products of direct primary deposition but are, instead, the products of *redeposition* is supported by a wealth of observations which anyone can confirm for himself. Granting this, the enormous volume of the graywacke suite in some sections means, therefore, that an equally large volume has been removed from some submarine locality. Though the turbidity currents at their site of deposition have no erosive power, the deposits themselves imply and carry evidence of marked erosive action elsewhere. The quantity of shale and siltstone fragments of intraformational origin is very large.

It is amazing that the obvious should have remained so obscure for so long. Although submarine mudflows and turbidity currents are effectively concealed from our view, their subaerial counterparts have been known for many years. These are, in the main, associated with volcanism and perhaps for this reason were unrecognized as such by the students of the sedimentary rocks.

The products of volcanic mudflows and the dust clouds of the *nuée ardente* type are the homologues of the pseudo-tillites and the conglomeratic graywacke of the geosyncline. The *nuée ardente* cloud is a turbidity current in the truest sense. It flows with great swiftness and fluidity down the mountain slopes.[4] B. N. Moore, writing in the *Journal of Geology* in 1934, made size analyses of the normal air-borne volcanic dust and of the deposits of probable *nuée ardente* origin. The composition and structure of the latter closely resemble that of the coarser conglomeratic graywackes (microbrec-

[4] Unlike subaqueous turbidity currents, the flow seems to be turbulent. That these clouds were true turbidity currents was pointed out some years ago by Bell (1942).

cias), whereas the former resembles the normal waterborne sands. It should be added that graded tuffs are exceedingly common. Those who doubt the erosive power of submarine mudflows or turbidity currents would do well to study their subaerial cousins (see Table 1).

In conclusion it can be said that the reality and importance of turbidity currents cannot be settled by discussion. Geologists must do what they have done in the past with some considerable success, namely, go to the earth for an answer. *Dig and discover.*

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REVIEWS

The Alkaline District of Alnö Island. By HARRY VON ECKERMANN. (*Sveriges Geologiska Undersökning*, Ser. Ca, no. 36.) 1948. Quarto, pp. 176; figs. 51; pls. 60, including 4 large colored maps in pocket. Sw. Kr. 10.

In a preface, the director of the Geological Survey of Sweden states that it is a great pleasure for the Survey to include in its publication series the results of Professor Eckermann's able and painstaking work (which has been printed at the author's personal expense) on one of the most interesting problems in Swedish geology.

Concentrically surrounding the intrusive alkaline rocks is an area of fenite, which is divided into six zones. The alkaline rocks embrace two types of paragenetic mineral assemblages: (1) alkali syenites: juvites, ijolites, malignites, melteigites, foyaites, tinguaites, nephelinites; and (2) ultrabasics: melanite-ouachitites, jacupirangites, kimberlites, alnoites.

In the center of the intrusion is a body of calcite carbonatite (*sövite*), with two types of dike, calcite carbonatite (*alvikite*) and dolomite carbonatite (*beforsite*).

The magmatic liquid, consisting mainly of carbonates, is supposed to have been rather cool (400–600° C.) with high internal CO_2(F, H_2O)-pressure, leading to a series of explosions and fracturing of the surrounding rocks (conesheets, ring-dikes, radial dikes) from three foci: at about 3 km. (*sövite*), 5 km. (*alvikite*), and 9 km. (*beforsite*) below the present erosion surface. The fenitization progressed simultaneously.

It will be hard to find a second rock province in the world as thoroughly investigated as the Alnö area. In addition to structural and tectonic studies, construction of maps and profiles, perfected by numerous borings, magnetic surveys, and mining operations, no less than 123 complete chemical rock analyses are published, together with extensive and illuminating petrochemical calculations.

T. F. W. B.

Mineral Resources of the United States. By the STAFFS OF THE BUREAU OF MINES AND THE GEOLOGICAL SURVEY. Washington, D.C.: Public Affairs Press, 1948. Pp. 212; charts 29; figs. 37; tables 41. $5.00.

This volume is the most comprehensive, authoritative, and up-to-date treatment of the nation's mineral resources available today. It is a reprinting, on a larger format, of pages 175–300 of *Hearings before a Subcommittee of the Committee on Public Lands, U.S. Senate, First Session on Investigation of the Factors Affecting Minerals*. A forward by Secretary of the Interior J. A. Krug has been added.

Following the Introduction and the Summary there are chapters on the "Method of Report," "Deficiencies of Data," "History of the Industry," and "Search for New Supplies." Separate chapters on "Solid Fuel," "Petroleum," "Mining," and "Treatment Trends" are followed by data on thirty-nine of the principal metallic, nonmetallic, and fuels resources. Some of the common raw materials, such as building stone, limestone, sand, gravel, clay, etc., which are plentiful in the United States, are not included.

The relative self-sufficiency of the nation with respect to its mineral resources is discussed and shown in the illustrations. The treatment and much of the data are familiar because of the many books and articles on the nation's resources published during the war. The present volume, however, is the most recent and comprehensive attempt, and the information given is based on the most reliable data available. The book contains a wealth of data, carefully analyzed and well presented.

A. F. HAGNER

An Introduction to Physical Geology, with Special Reference to North America. By WILLIAM J. MILLER. 5th ed. New York: D. Van Nostrand Co., Inc., 1949. Pp. x+482; figs. 397.

This fifth edition of a textbook which has been popular for a third of a century would not shock the reader who perused the first edition and has read no geologic literature since then. It is an excellent proof of the longevity of the ultra-conservative—a phenomenon with which paleontologists are well acquainted.

The first edition's 351 illustrations grew to

397 in the third and have remained constant since then. The size and the quality of the illustrations have grown from the beginning, but in many cases merely through the use of a larger reproduction of the same photograph or drawing. A large part of the "revision" has consisted merely of rearranging the old material (whether illustrations or text), a little better drawing of the same thing, a somewhat more "streamlined" wording, or a more recent statistical figure inserted into an old sentence. In other words the revisions—especially in the fourth and fifth editions—are superficial and insignificant. Typical are the picture and paragraph on Parícutin, and the even briefer mention of uranium.

The outstanding example of the book's obsolescent condition is the section on the "cause of volcanic activity." The author quoted it from one of his other books for his first edition—it remains unchanged thirty-five years later.

The book is purely descriptive and preceptive: it asks no questions except rhetorically; it leaves no problems unsolved. Perhaps the reviewer errs if he expects of the fifth edition what the others did not produce, but he feels that the book fails to give today's college student the kind of textbook he expects and should have.

MICHAEL S. CHAPPARS

Proceedings of the Federal Inter-agency Sedimentation Conference. Edited and prepared for publication by the BUREAU OF RECLAMATION, UNITED STATES DEPARTMENT OF THE INTERIOR. Washington, D.C., 1948. Pp. x+314.

The Subcommittee on Sedimentation of the Federal Inter-agency River Basin Committee sponsored a conference on sedimentation at Denver, May 6–8, 1947. Participating in the conference were representatives of the Soil Conservation Service, the Forest Service, the Corps of Engineers, the Coast and Geodetic Survey, the Bureau of Reclamation, the Geological Survey, the Office of Land Utilization, the Bureau of Indian Affairs, the Federal Power Commission, and the Tennessee Valley Authority.

Sessions were devoted to agency sedimentation programs; determination and interpretation of sediment load of streams; reservoir sedimentation; sedimentation problems in rivers, floodways, and canals; laboratory investigations; and sedimentation control. The papers presented were largely "practical" in character and dealt with the engineering aspects of sedimentation. However, some of the papers will be of interest to students of sediments and sedimentary deposits. Of special interest is the paper on the mechanics of sediment transport by V. A. Vanoni and that of C. S. Howard on the bottom withdrawal-tube method of size analysis.

F. J. P.

The Coast of Northeast Greenland, with Hydrographic Studies in the Greenland Sea. The Louise A. Boyd Arctic Expedition of 1937 and 1938. By LOUISE A. BOYD, with contributions by R. F. FLINT, J. M. LEROY, H. J. OOSTING, F. A. BUHLER, F. E. BRONNER, A. J. HILFERTY, ALICE EASTWOOD, and the U.S. COAST AND GEODETIC SURVEY. (American Geographical Society Special Publication 30.) New York: George Grady Press, 1948. Pp. 339; figs. 194; pls. 12. $6.00.

This publication, which was delayed because of the war, contains the scientific results of Miss Boyd's third and fourth expeditions into the Arctic. The region covered includes the northeastern coast of Greenland between King Oscar Fiord and Isle de France, Jan Mayen Island, and routes across the Greenland Sea. Separate reports are given on the following subjects: the expeditions and equipment; geology; botany; hydrographical, topographical, and photographic surveys; current observations; tide observations; and magnetic observations.

Geologists will be interested particularly in the geologic reports by R. F. Flint and F. E. Bronner and in the hydrographic maps. In the present review, only Flint's report on the geomorphology and glacial geology can be considered further.

Two conspicuous geomorphic elements of the Greenland coastal region are emphasized: (1) a mature, seaward-sloping highland surface, formerly considered a peneplain, and (2) a system of deep fiords and valleys entrenched below its surface. The age of the upland surface and the time of valley-cutting remain uncertain. The glaciers of the region are described in greater detail, with the following general conclusions: (1) foliation, probably of secondary origin, is present in most of the glaciers and commonly has a synclinal structure; (2) many "moraines" with ice cores cause landsliding and development of ridges which are unrelated to the glacier's regimen; (3) dike ridges on some glaciers are con-

sidered fines squeezed up along fissures; (4) composite valley profiles suggest at least two major glaciations; (5) the rarity of cirques below the highland level indicates that the regional snowline may be no higher now than when the cirques were formed; (6) since the last general deglaciation there appears to have been a recent readvance (possibly mid-nineteenth century), followed by the deterioration now in progress; (7) the shrinkage of glaciers within historic time is probably due more to surface ablation than to retreat of ice margins. Numerous observations of emerged strandlines are tabulated, but correlations are not attempted. A final section of the report deals with weathering, mass-wasting, and eolian features.

L. H.

Structural History of the East Indies. By J. H. F. UMBGROVE. London and New York: Cambridge University Press, 1949. Pp. xii+63; figs. 67; pls. 10; tables 4. $4.00.

The widely scattered literature on the fascinating geology of the Dutch East Indies makes it increasingly difficult for the outsider to follow the progress of work in this large region, although in recent years several summaries in English have been published. The present book is essentially "the text of a series of lectures which were delivered to an audience of students of geography, geology, and geophysics in the University of Cambridge, in May 1946." Umbgrove presents the essential geological facts and theories in clear, simple language and has added a great many excellent text figures and plates to facilitate the geographical references and significant relationships between the principal areas.

The lectures begin with one on "The Shallow Seas," in which the results of the Snellius Expedition and other oceanographic and sedimentologic studies are summarized. The influence of ocean currents and vertical movements of the ocean floor on the distribution of sediment is beautifully shown. In the second chapter, on "Deep-Sea Basins and Troughs," the relief features of the several zones are outlined. Important for the type of sediment is the oxygen content of the water. This again is determined by the depth below sea level at which polar oxygen-rich water can enter each trough. A table and several figures show how close the correlation is. The rate of sedimentation in some basins is very slow. Ash from an eruption in 1815 is believed to be still in the process of sinking. However, elsewhere a rate of sedimentation of about 5 cm. per century has been calculated, and at this rate some basins could be filled up in about ten million years, unless vertical movements interfere.

The famous volcanoes of the East Indies are briefly treated in the third chapter, with special reference to their relation to fracture zones and to theories by Van Bemmelen and others concerning the genesis of the basaltic and more potassic clans. The arrangement of volcanoes along distinct belts leads, in the fourth chapter, to a discussion of the "Structural Zones." The historical development of the individual blocks is traced, and maps and charts help to portray the varying sequences of events in each. Post-Cretaceous movements and periods of erosion and deposition are sketched in considerable detail. Figure 49, on page 41, gives in appropriate symbols the history of thirty-five different areas. Apart from the elongated shape of the belts, the pattern of the mobile blocks resembles strikingly the geology of the Coast Ranges of southern California. The thickest Tertiary column seems to be near Pasir, eastern Borneo, which is listed as 15,000 meters.

For an explanation of the complicated history of the East Indies, the author turns to the well-known seismic and gravimetric data. In the concluding chapter, "Geophysics," he adopts Vening Meinesz' hypothesis of a down-buckle of sialic material, the gradual evolution of which is believed to account for the deformation, subsidence, uplift, and volcanism of contiguous crustal belts. The distribution of shallow and deep earthquake foci is also believed to fit into this picture.

Those who find it difficult to accept this picture of the Dutch scientists will probably agree with the author when he says, on the last page: "Personally, I am convinced that it is only of temporary value, something like a working hypothesis which has to be discarded as soon as new facts are revealed which are incompatible with our present views." This brief summary of the present state of knowledge, in which some of the more important references are listed, should help us all understand the nature of the problems of this region which challenges geologic thinking.

R. B.

"Report of the Committee on the Measurement of Geologic Time, 1947–1948." JOHN PUTNAM MARBLE, *Chairman*. Presented for pub-

lication to Division of Geology and Geography, National Research Council, March, 1949. Mimeographed. Washington, D.C. Pp. 77. $1.00.

These reports are published annually with the purpose of correlating and summarizing the widespread work done in fields touching upon geologic time measurement. The present edition consists of a summary report by John Putnam Marble, and six Exhibits. Exhibit A is a paper by Arthur Holmes listing a series of analyses of uraninite and monazite from the Tanganyka Territory and India. Exhibits B–E consist of the following papers: (B) "The Necessity for Different Values for Ratio Pb/U of the Crust and the Rest of the Planet," by Juan Manuel Lopez de Azcona (translated by A. H. Marble and J. P. Marble); (C) "Review of Work in Japan," by John Putnam Marble; (D) "Archaeological Ages by Natural Radiocarbon Content," by W. F. Libby with comments by R. F. Flint; (E) "Preliminary Report on Determining the Age of Rocks by the Lead-Uranium Ratio of Zircon, Apatite, and Sphene from the Rocks, Using Alpha Counting and Spectrographic Methods," by E. S. Larsen, Jr., N. B. Keevil, and H. C. Harrison. Exhibit F, compiled by John Putnam Marble, consists of about forty-seven pages of bibliography of both foreign and United States literature. Each reference is supplied with a short abstract. Although Dr. Marble admits that the bibliography is not complete, it is certainly a useful and concise guide. It is noteworthy that the list has not been restricted to papers having direct application to geologic time measurement. Indeed, the references are characterized by great diversity, the philosophy evidently being, and rightly: "Who can tell? It may be useful."

LEON ATLAS

Readings in the Physical Sciences. Edited by HARLOW SHAPLEY, HELEN WRIGHT, and SAMUEL RAPPORT. New York: Appleton-Century-Crofts, Inc., 1948. Pp. xiii+501.

The stated purpose of this book is to "bring to the general student understanding of the fundamental nature of the physical world in which he lives and of the skills by which this nature is discerned." The careful selection of excerpts from the writings of scientists, philosophers, and laymen has resulted in a volume which should be valuable to students and fascinating reading both for the general public and for scientists.

The book is made up of six sections: "Science and Scientific Method," "Astronomy," "Geology," "Mathematics," "Physics," and "Chemistry." Each begins with an introduction and closes with a bibliography. The latter does not merely repeat the references from which the quotations are drawn but consists of a fairly extensive list of works which will enable the interested reader to pursue further knowledge as his taste and will dictate. As an example, the section on geology consists of ten selections totaling seventy-six pages and a bibliograph of seventy-two titles.

Perhaps the customary facile division of science into applied and fundamental (or "basic," or "pure") has been unfortunate. Some "pure" scientists have come to believe their endeavors to be on a higher moral plane than those researches which were aimed at immediately practical targets and to consider a professor who writes "popular" articles as downright meretricious. Fortunately, some leading scientists have proved to be eminently capable of conveying scientific ideas to the public. They, together with other men who were fluent professional writers imbued with the scientific spirit and who had learned restraint of expression along with their facts, have made possible the compilation of this fascinating volume.

That the sciences are really one is shown again and again by many authors. Applied science rests on pure research as on a foundation, and, as William F. Durand says, ". . . the ideal is and must be that all knowledge should be turned in some way into expression in terms of service to humanity."

Today the struggle for the world lies between a science that is government supported, directed, regimented, and given an ideological slant, on one side, and a science partly subsidized and directed by government, but otherwise free, on the other hand. This book affords some glimpses into the workings of untrammeled scientific research which should give the reader confidence as to the final outcome of this quiet cosmic struggle.

MICHAEL S. CHAPPARS

Landscape as Developed by the Processes of Normal Erosion. By C. A. COTTON. 2d ed. New York: John Wiley & Sons, Inc., 1948; printed

by Whitcombe & Tombs, Ltd., Christchurch, New Zealand. Pp. 509; figs. 375. $10.00.

Landscape is concerned with the origin of landforms in moist climates and is one volume in a trilogy on geomorphology, which includes *Climatic Accidents in Landscape Making* (1942), dealing with features in arid and glaciated regions, and *Volcanoes as Landscape Forms* (1944). Taken together, they represent the most comprehensive treatment of geomorphology in English and virtually the only text available for an advanced course in that subject.

The second edition of *Landscape* is in large part a new publication: there are about two hundred pages added to the text, seventy-five additional illustrations, and a new chapter on "Limestone Caverns." Also there are numerous revisions of the text, and many old illustrations have been replaced by new. References have been brought up to date to 1947 and are given at the end of chapters instead of as footnotes. The Index has been enlarged from nine to sixteen pages. A particularly helpful innovation is that of section headings within chapters.

Those familiar with the older edition will find little change in the over-all organization and thoroughly Davisian treatment of subject matter. The most apparent changes are those in which extended treatment is given to such topics as frozen-ground features, climatically controlled terraces, terrace slopes, fault and fault-line features, and karst forms.

It remains a pre-eminent text in its field and, because of its literary qualities, is outstanding among geologic works in general.

L. H.

Soil Science Society of America, Proceedings 1947. Soil Science Society of America, G. G. Pohlman, *Secretary-Treasurer*, Morgantown, W. Va. Geneva, N.Y.: W. F. Humphrey Press, Inc., 1948. Pp. 539. $7.50.

Section V of this report (pp. 381–464) includes fourteen papers on "soil genesis, morphology, and cartography," which emphasize the significant contributions being made by soil scientists to essentially geological problems. As indicated by the following selected titles, the papers are of special interest to glacial geologists, sedimentologists, and geomorphologists:

WASCHER, H. L.; HUMBERTY, R. P.; and CADY, J. G. Loess in the Southern Mississippi Valley: Identification and Distribution of the Loess Sheets.

ANDREW, L. E., and RHOADES, H. F. Soil Development from Calcareous Glacial Material in Eastern Nebraska during Seventy-five Years.

BOURBEAU, G. A., and BERGER, K. C. Thin Sections of Soils and Friable Materials Prepared by Impregnation with the Plastic "Castolite."

KRUSEKOPF, H. H. Gumbotil—Its Formation and Relation to Overlying Soils with Claypan Subsoil.

WHITESIDE, E. P. Preliminary X-Ray Studies of Loess Deposits in Illinois.

HUTTON, CURTIS, E. Studies of Loess-derived Soils in Southwestern Iowa.

RIECKEN, F. F.; ALLAWAY, W. H.; and SMITH, GUY D. Some Soil Classification and Mapping Problems in the Wisconsin Drift Area of Iowa.

EKERN, P. C., JR., and MUCKENHIRN, R. J. Water Drop Impact as a Force in Transporting Sand.

L. H.

A Glossary of Scientific Names (Chiefly of Fossil Invertebrates). By STUART A. NORTHROP. Albuquerque: University of New Mexico Press, 1949. Pp. 71. 16 mo.

This little pamphlet gives the derivations and meanings of about eighteen hundred generic and specific names, mostly of invertebrate fossils. An Introduction explains the nature and construction of such names and a short list of references. It should be of considerable interest and usefulness to students of paleontology.

J. M. W.

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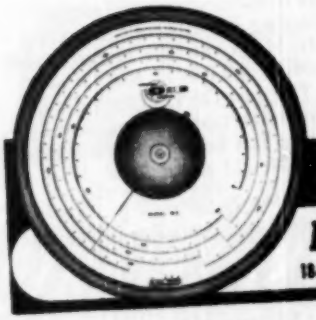

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CONTENTS

		Page
FOREWORD	By John T. Galey	1395
SUMMARY OF MIDDLE ORDOVICIAN BORDERING ALLEGHENY SYNCLINORIUM	By Marshall Kay	1397
CAMBRIAN AND ORDOVICIAN ROCKS IN MICHIGAN BASIN AND ADJOINING AREAS	By George V. Cohee	1417
NEW YORK SUBSURFACE GEOLOGY	By E. T. Heck	1449
SUBSURFACE TRENTON AND SUB-TRENTON ROCKS IN OHIO, NEW YORK, PENNSYLVANIA, AND WEST VIRGINIA	By Charles R. Fettke	1457
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